

# Psychological Review

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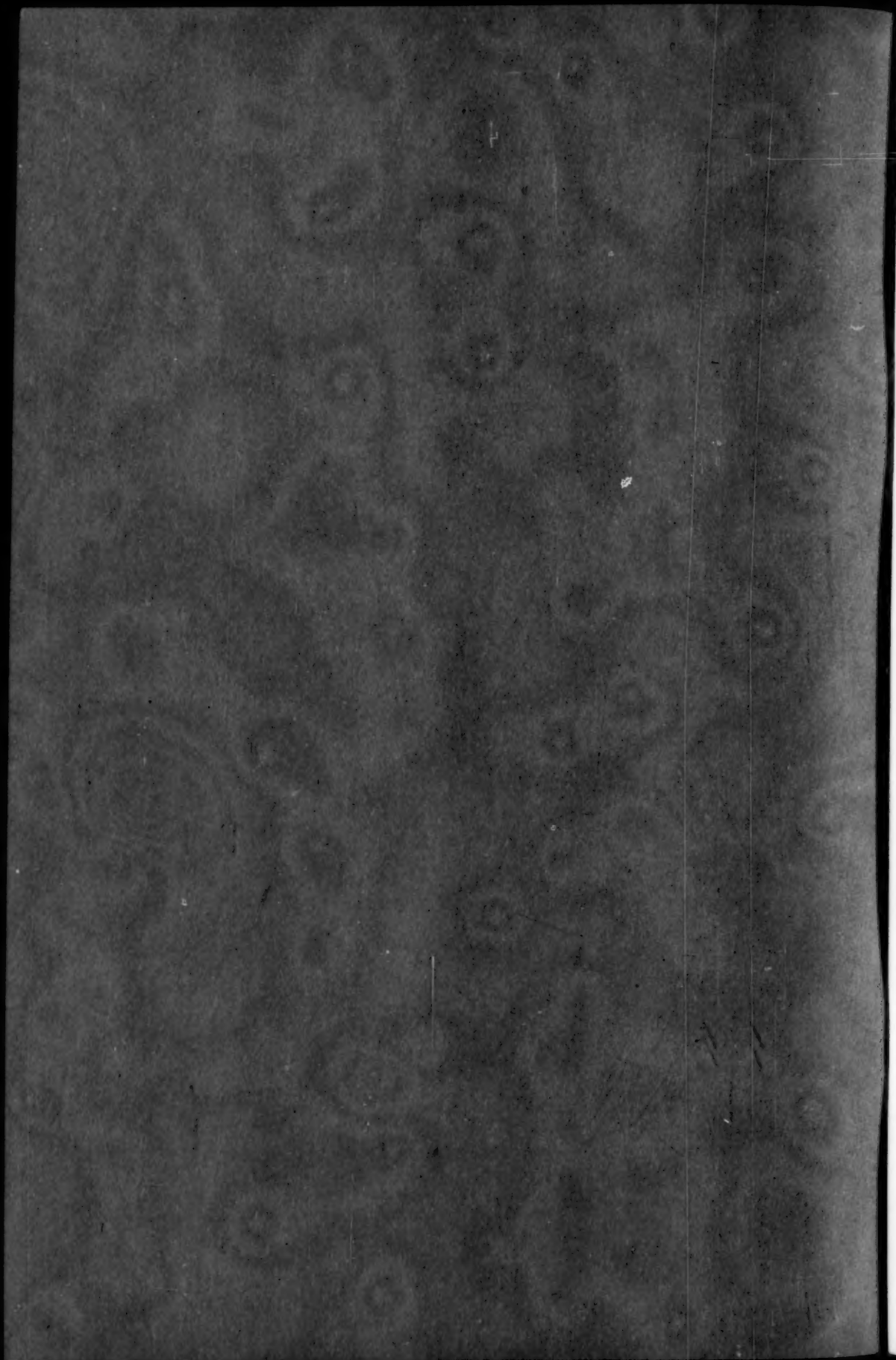
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# THE PSYCHOLOGICAL REVIEW

## THE ROLE OF SECONDARY REINFORCEMENT IN DELAYED REWARD LEARNING

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### I

One of the most puzzling problems facing the learning theorist has been that of explaining how a reward or goal situation which occurs sometime after a stimulus response event (*S-R* connection) is apparently able to work back and strengthen it. A number of attempts (14, 15), primarily quasi-physiological in character, have been made to answer this problem, but none has been satisfactory in the sense that it has led to experimentally testable implications which could be verified. One of the main factors responsible for this failure, in the opinion of the writer, is that the learning theorist, until recently, was much too inclined to conceive of explanation in psychology as necessarily consisting in an account of the neurophysiological mechanisms mediating the behavior. More recently, however, psychologists have come to realize that explanation of behavioral events does not *necessarily* involve reduction to its physiological determinants. Accepting the task of the psychologist as being that of establishing interrelationships (laws) within the realm of those observable events (responses and environmental situations) that are his particular concern, these theorists have introduced a variety of new types of constructs to aid them in their purpose (1, 12).

In the case of learning phenomena, a number of theoretical interpretations have recently been offered which make little or no use of neurophysiological concepts. Thus Hull (6) and Tolman (16) conceive the task of the learning psychologist as being that of formulating the laws relating the behavioral changes which occur with practice to the particular environmental (stimulus) conditions, past and present, that play a role in these changes. To this end both have found it necessary to introduce theoretical constructs, or what they term intervening variables, which serve to bring into relation with one another the dependent *R* variables on the one hand, and the independent *S* variables on the other.

In the case of Hull's theorizing, a hypothetical variable 'habit strength' (*H'*) is defined in terms of certain experimental variables, *e.g.*, number of times the response has occurred and been followed by a reinforcing or goal object, the magnitude of the goal object, the time of its delay, etc. If one wishes, this theoretical variable can be thought of as representing a hypothetical state or condition in the organism which is the product or the result of the operation of these environmental events in the past. In other words, it represents a kind of learning factor based on experience. In a like manner, other

hypothetical factors such as drive (*D*), inhibition (*I*), excitatory potential (*E*), etc., have been introduced. The observable response measures are ultimately stated to be definite (hypothetical) functions of the final member in the chain of these intervening constructs.<sup>1</sup>

The treatment of the problem of the delay of reinforcement within such a theoretical framework has involved attempts to guess at the relationships holding between one or another of these intervening variables and the time of delay variable. Thus in the original formulation of his *goal gradient hypothesis* Hull (4) proposed the principle that the degree or extent of learning or conditioning of reactions to their co-incident stimuli depends upon the remoteness (in time or space) of the subsequent reward. On the basis of certain existing experimental data, he assumed this gradient to be logarithmic. Subsequently Hull (5, 6) modified this *goal gradient principle*, stating it more specifically in terms of assumed effects of the experimental variable on the theoretical learning factor, habit strength (*H*). According to this restatement, the maximum (*M*) to which the habit strength can increase with a given reinforcing agent is, other things equal, an exponential function of the time of delay of the reward.

More significant than the change from a logarithmic to an exponential function, however, is the treatment in

this new formulation of the *goal gradient* as a secondary rather than a primary principle. As a matter of fact, Hull's original *goal gradient hypothesis* was not a theory to account for the variation of learning with the time of delay. It simply assumed this, *i.e.*, that the amount of learning varied (logarithmically) with delay of reward, and then went on to show how on the basis of this assumption certain phenomena in the maze learning of animals could be deduced (explained). Thus the elimination of blind alleys, the quicker elimination of long than short blinds, the quicker elimination of blind alleys near the goal than those farther removed, and other implications, were shown to be explainable on the basis of this *goal gradient principle*. In all these discussions this principle was taken as primary and a number of secondary phenomena were derived from it. In the *Principles of Behavior*, on the other hand, the *goal gradient* is shown to be a secondary principle derivable from other, more basic principles. The remainder of this article will be concerned with this new formulation and certain suggestions for its modification which eliminate the problem of having to explain how rewards act backwards.

## II

The hypothesis that delay of reward has a gradient effect on the rate and progress of learning was first suggested by Thorndike (13) in 1913. Within the next two decades a number of experimental studies (3, 11, 19) definitely established the fact that as the reward was delayed its effectiveness in producing learning decreased in a gradient fashion. Wolfe's study (19) indicated that while the gradient fell very rapidly within the first minute it apparently extended well beyond this period since delays of even 10 and 20 minutes in length

<sup>1</sup> The writer has previously called attention to the fact that Hull's theoretical constructs should not be interpreted as being neurophysiological in nature, although they sound very much as though they might be (12). As actually employed by Hull, they are defined in terms of mathematical equations which relate them to such experimental variables as number of trials, time of deprivation of a goal object, time of delay of the reward, etc. The additional statements Hull usually makes as to their neurophysiological locus are superfluous so far as his theorizing is concerned.



were effective in bringing about some learning.

Quite in contrast to these earlier studies which exhibited long delay gradients, two recent studies by Perin (7, 8) have reported much shorter reinforcement gradients. Extrapolation of the curve fitted to one set of Perin's data indicated that the effectiveness of the reward fell to zero at a delay of between 30 and 40 seconds, while the second study suggested the possibility of an even shorter gradient.

Analyzing the various experimental findings, Hull (6) was led to conclude that a major factor operating in the delayed reward situation is the degree to which conditions are favorable for secondary reinforcement.<sup>2</sup> If the situation is one which permits any of the stimulus events regularly occurring in the delay interval to acquire secondary reinforcing properties, then it is as if reward is not actually delayed, since instead *immediate secondary* reinforcement occurs. Thus Hull explained the failure of two early studies (17, 18) to find any difference in the rate of learning of a delayed and a non-delayed group as due to the fact that the animals were admitted *immediately* into the feeding compartment following the correct response, although they were prevented from eating for the delay period. The goal box and odor from the food container, however, provided immediate secondary reinforcement as these cues had acquired this property either from the preliminary training or previous experience with the sight and odor of food.

Hull pointed further to the fact that in studies in which a gradient effect in learning was obtained, the delay occurred in a separate detention compart-

ment outside of the food box. Under these conditions the secondary reinforcing cues of the goal box were also delayed. Because of their consistent temporal and spatial association with the goal box and food, however, the stimulus cues in the delay compartment also come to acquire secondary reinforcing properties. Once they do, there is no longer any delay of the secondary reward in the situation.

Perin's studies, which were carried out as a part of a coordinated research program under Hull's direction, were specifically designed with a view to eliminating secondary reinforcement. Noting that Roberts' (11) study of delayed reward had differed from others in that the learned response occurred in the delay compartment rather than prior to entering it, Perin proposed to use a single compartment which would serve all three functions: (1) as a problem box, (2) as a delay chamber, and (3) as a food box. Perin writes concerning this procedure: "The stimulus environment of the animal would be much more constant during the delay interval, and the train of external stimuli leading to the food reward, characteristic of the maze situation, would be eliminated: This would materially simplify the problem of interpretation by eliminating an irrelevant factor" (7, p. 38).

If this explanation of Perin as to why secondary reinforcement is supposedly eliminated or reduced in this experimental situation is not too clear or convincing, his results, nevertheless, were in agreement with the inference that such would happen. Both experiments indicated that learning would not occur if the food were delayed longer than 30-40 seconds.

### III

Largely on the basis of the contrasting gradient lengths obtained in these

<sup>2</sup> According to the principle of secondary reinforcement, stimulus cues which have been closely and consistently associated with a reinforcing state of affairs themselves acquire reinforcing properties (6, p. 95).

various studies, Hull was led to suggest the new formulation of the goal gradient hypothesis which appears in his *Principles of Behavior*. According to this conception there are two distinct gradients involved in delayed reward situations: (1) a basic or primary gradient of reinforcement, which in the case of the white rat extends over a relatively short period of time, possibly as brief as 30 seconds; (2) a more extended, secondary gradient which results from the joint action of the primary gradient and the principle of secondary reinforcement. The latter, termed the *goal gradient*, is assumed by Hull to be exponential in form and to be limited in its extent by the degree to which conditions are or are not favorable for the development of secondary reinforcement.

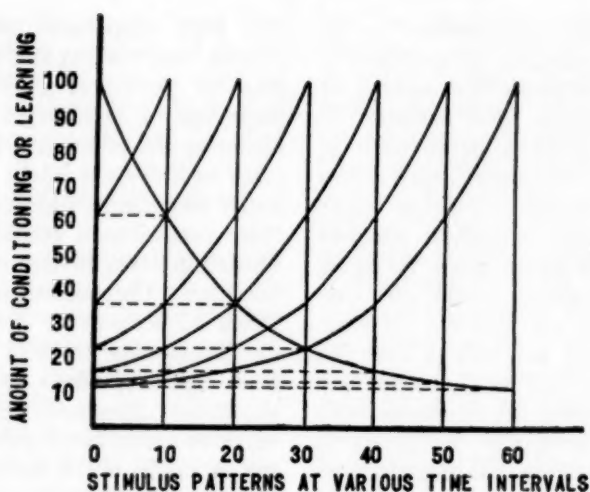
The manner of development of the goal gradient is described tentatively by Hull as involving the progressive movement forward of secondary reinforcement from the point of primary reward. At first, stimuli within the range of the primary gradient acquire secondary reinforcing properties and these in turn serve to establish secondary reinforcing powers in still more antecedent stimulus cues. Each of these secondary reinforcing stimuli is assumed to have its own short (primary) gradient of reinforcement and the goal gradient is conceived of as resulting from the summation of these short, overlapping component gradients. The further removed the response is from the occurrence of the primary reward, the weaker will be the strength of the secondary reinforcement and the longer will be the time taken for it to work forward to a point within the range of primary reinforcement of the response. Hence the rate and limit of learning will decrease as the magnitude of the delay is increased.

This formulation of Hull is seen to be most plausible in the case of a de-

layed reward situation involving an actual spatial or distance delay such as in the typical serial maze situation. It is easy to see how a longer time would be required for the secondary reinforcement to move forward the longer the delay path, and it is not unreasonable to conceive of the secondary reinforcement becoming weaker the greater the number of steps (distance) it is removed from the primary goal box. The occurrence of a learning gradient with delay of reward in such a situation would appear to be quite consistent with this theoretical picture.

In situations in which the delay of reward involves primarily time and not distance, however, this explanation of a gradient of learning is in need of supplementation. Thus it is evident that different amounts of *temporal delay* in a delay box do not require any more steps in the conditioning process by which the cues of the delay chamber acquire secondary reinforcing properties. Hence it should not take any longer for the secondary reinforcement to move forward to the delay chamber under the different time delays. In spite of this, the experimental evidence definitely points to the occurrence of a gradient of learning in such circumstances.

There are a number of plausible ways of accounting for this apparent inconsistency. For example, one possibility is that the cues in the delay boxes possess weaker secondary reinforcing properties the longer the delay time for the reason that these cues may occur several times during the delay period without being reinforced by the food. Different delay periods would thus involve different proportions of reinforcement of these cue stimuli; the longer the delay the smaller the proportion of reinforcements. Unfortunately there is no evidence available as to the strength of secondary reinforcing cues which have



been established with varying proportions of reinforcements.

A second hypothesis as to the mechanism responsible for a learning gradient in the case of temporal delays of reward involves the concepts of stimulus pattern and generalization. Thus we may assume that it is the particular stimulus pattern that occurs coincidentally with the food reward that acquires secondary reinforcing properties and that this conditioning generalizes to the stimulus patterns preceding it in time according to some gradient. The figure attempts to show how this theoretical schema would lead to the implication of a gradient of learning with temporal delay of the reward. In this graph the curves falling from the right side of the graph to the left are hypothetical generalization curves<sup>\*</sup> for different delay periods. Thus if the secondary reinforcing strength of the stimulus pattern occurring after a delay of 60 seconds is assumed to have reached 100, generalization to other

stimulus patterns preceding it in time is assumed to take place according to the generalization curve falling from this point. Thus, the reinforcing strength of the stimulus pattern occurring at the time of the learned response is given by the value of the generalization curve at zero time interval. In the case of the 60-second curve this value is 11.6. Similar generalization curves are shown for cases of food delay of 10, 20, 30, 40 and 50 seconds. If we now make the assumption that the strength of the immediate secondary reinforcing cues (*i.e.*, the values of the curves at zero delay) determines the relative amount of learning that will occur, we obtain the gradient curve falling from the left to right side of the graph.

The changing stimulus pattern following the critical, to-be-learned response may be envisaged in terms of the external environmental cues *plus* the time interval as such. Thus, in the case of a delay of 60 seconds,  $S_{\text{Delay Box} + 60 \text{ seconds}}$ , is reinforced by food, whereas all other stimulus complexes (*e.g.*,  $S_{\text{DB} + 50}$ ) are not. Another and possibly more promising method of conceiving of the changing stimulus complex during a delay period involving a

<sup>\*</sup> The equation describing these curves of generalization is as follows:  $H$  (amount of learning) =  $100e^{-.03T}$ , where  $T$  is the time between the occurrence of the learned response and the subsequent reward.

constant external environment is in terms of the changing proprioceptive stimulus cues following the making of the correct response. For example, in the simple T maze situation such as used by Wolfe, the proprioceptive cues following the correct response (e.g., left turn) acquire secondary reinforcing properties whereas those following the incorrect response (right turn) do not, as they are not followed by food. Presumably these two sets of cues are most different from one another immediately following the responses, with the difference gradually decreasing to zero in time. Differential secondary reinforcement based on such differential proprioceptive cues presumably can occur in the delayed reward situation for at least the length of time the two proprioceptive patterns remain discriminatively different from one another. Of course, in the Wolfe study the two delay boxes provided differential cues from the external environment; but it is difficult to see how these cues alone could have provided for a gradient of learning with increase of the delay.

In an attempt to eliminate all possibility of differential secondary reinforcement from the external environment, Perkins (9) recently conducted an experiment with a special type of simple T maze. The maze was an enclosed one, with the floor and top consisting of opal flashed glass, thus precluding visual cues from outside the maze. Throughout the training period the two delay boxes were shifted systematically from one side to the other. This procedure meant that cues from both of the two delay boxes were followed by food half the time and half the time not. But more important is the fact that even though both delay boxes should acquire some secondary reinforcing properties they do not provide any *differential* secondary reinforcement, for the reason that they fol-

low both responses indiscriminately. Perkins also rotated the position of the maze in the room 180 degrees at the beginning of each day's run so as to eliminate the possibility that some constant auditory cue might provide differential secondary reinforcement. Under these conditions a gradient of learning similar in form to that of Wolfe's was obtained. Comparison of the learning (with 45 seconds of reward delay) of two groups, one of which had the conditions described above and one in which the delay chambers were not shifted, revealed a significant difference in learning in favor of the latter. Apparently the additional differential secondary cues provided by the external environment facilitated learning.

But more crucial evidence supporting the notion that delayed reward learning is in part a function of secondary reinforcement from differential internal (proprioceptive?) cues is the experimental finding of Gulde (2) and Riesen (10) that when care is taken to delay the consequence of both correct and incorrect responses as little as four to five seconds, animals (rats and chimpanzees) are unable to learn in the discrimination learning situation. This type of experimental situation simply goes one step further than the Perkins experiment in controlling the differential environmental cues, and hence differential secondary reinforcement, following the critical responses. In discrimination learning the stimulus cues (e.g., white and black) are shifted from left to right in a random manner. That is to say, the choice of the positive, correct stimulus sometimes involves a left turn and at other times a right turn, and similarly, choice of the negative stimulus involves both responses equally often. Likewise, responses to both cues are followed equally often by both delay boxes. In this situation, then, there is neither a characteristic internal proprio-



ceptive cue nor an external cue that consistently follows either one of the responses and can thus serve as a differential secondary reinforcer. In such a situation we should expect the limits of learning with delay of reward to be relatively uncomplicated by secondary reinforcement.<sup>4</sup>

The fact that no learning occurs with delays as short as five seconds in the discrimination situation suggests that the primary gradient of reinforcement, if there is such a thing at all, is much shorter than the 20 to 30 seconds inferred by Hull on the basis of the Perin studies. Indeed, it would not seem unreasonable to hypothesize that there is no primary gradient of reinforcement, but that all learning involving delay of the primary reward results from the action of *immediate* secondary reinforcement which develops in the situation. Factors affecting the development of this secondary reinforcement would then be considered as responsible for all gradients of learning found with delay of reward. Such a hypothesis, it will be observed, eliminates the necessity of explaining how the reward seemingly acts backward over time to influence something which occurred earlier.

Returning to the Perin studies, it will be noted that the experimental method of having the various responses, including the correct one, occur in the goal

box has much the same effect as the technique employed by Perkins in his T maze. In effect, it eliminates *differential* secondary reinforcement from the *external* environment, *i.e.*, the goal box, for all responses made in the situation are followed by more or less the same set of visual cues. Thus they all receive secondary reinforcement.

A possible basis for differential secondary reinforcement in the Perin situation might be the different pattern of proprioceptive cues resulting from the different possible responses. Thus the particular pattern (or set of patterns) of proprioceptive cues that results from the act of depressing the bar presumably continues as a distinctive, but changing, trace for some time following the response. The trace of this pattern which is coincident with the delivery of the food will be the one, theoretically, that acquires secondary reinforcing properties. However, insofar as this trace pattern is within the range of the generalization gradient of the particular pattern occurring at the time of the response, the latter will also acquire secondary reinforcing properties. The longer the period of time elapsing between the act of depressing the bar and the receipt of the food, the greater will be the difference between the proprioceptive patterns and the less the generalization of secondary reinforcement. According to this hypothesis the gradients of learning found by Perin presumably reflect this generalization gradient.<sup>5</sup>

#### IV

In summary, the interpretation that learning under conditions of delay of

<sup>4</sup> As a further test of the role of such differential proprioceptive cues, another investigation was instituted in the Iowa laboratory in 1942 by Robert Grice. This experiment attempted to obtain delayed reward learning in a white-black discrimination situation by having the subjects, white rats, make some distinctively different motor response to the positive and negative stimuli. Thus the situation was arranged so that the rats had to duck under a hurdle in the positive white alley and clamber over a fence in the negative alley. Unfortunately, call to military service interrupted the study (a doctoral thesis) just after it was started and it is only now in the process of being completed.

<sup>5</sup> An interesting implication of this hypothesis which would lend itself readily to experimental test is that the less variation that occurred in the motor pattern of the response being learned, the more effective will the learning be for a given amount of delay.

primary reward involves a backward action of the goal object on the preceding stimulus-response event is rejected. The hypothesis suggested as an alternative to this conception is that all such learning occurs as the result of the development of secondary reinforcement, the action of which is conceived to take place immediately upon the occurrence of the response. A prominent aspect of this theory is the concept of *differential secondary reinforcement* based either on cues from the external environment or internal cues such as proprioceptive stimulation.

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# OUTLINE OF A DESCRIPTIVE AESTHETICS FROM A STRUCTURALIST POINT OF VIEW

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## I. DESCRIPTIVE AND EXPLANATORY AESTHETICS

The aesthetic experience may be roughly characterized (though not indeed defined) as a special phenomenon of liking or disliking. And in this action of liking or disliking an object we may consider (a) either *how* this phenomenon of liking arises and develops, or (b) *why* I like or dislike a given thing. Thus we might call (a) *Descriptive Aesthetics*, what we were doing in the first case, and (b) *Explanatory Aesthetics*, what we were trying to achieve in the second case. An ideal science of Aesthetics would never be complete without the explanation of different factors that influence the aesthetic experience; but a Descriptive Aesthetics may precede and help the explanatory task.

In this descriptive part of Aesthetics, even more than in the other one, we must deal with several facts which are not exclusively aesthetic but fall under certain general laws of psychology; and so in Descriptive Aesthetics we may study the aesthetic facts of the contemplation and enjoyment of art merely as *special cases of the more general psychological phenomena of perception and feeling*. It will be, therefore, a new field where the main principles of Gestalt psychology might be profitably applied.

Some attempts to explain certain aesthetic phenomena from the point of view of the Gestalt School, or from others akin to it, have already been

\* This article is the result of a research in general psychology at Harvard University under the supervision of Dr. John G. Beebe-Center.

made by von Allesch, Ogden, Langfeld and others. Nevertheless we must grant that their attempts are much more related to Explanatory Aesthetics than to a descriptive conception of this science. Von Allesch states that a piece of art must be positively evaluated when it possesses 'intentional unity,' 'compactness,' 'intentional breadth' and 'impres-siveness or intensity' (3). Langfeld, in his book *The Aesthetic Attitude* (32), has explained in detail the process of discovering a three-fold unity in a piece of art—of form, of content, and of content and form—as the main source of aesthetic enjoyment. Ogden, in his *Psychology of Art* (38), supplies excellent material for Descriptive Aesthetics, but his evaluation of 'Dynamic and Static Symmetry' (39, p. 206) falls rather under Explanatory Aesthetics. Arnheim (4) and Eysenck (18) have also tried to apply the principles of Gestalt psychology to the solution of the aesthetic problems. They are, however, primarily interested in the problem of explaining the causes of aesthetic pleasure. Koffka (29), one of the three creators of the Gestalt movement, has also made very interesting suggestions concerning Aesthetics from the point of view of this school, but the relations between the artist and the world have been the main subject of his work. It is evident that there is a clear relation between these conceptions of beauty and a 'Structural Descriptive Aesthetics.' Nevertheless we must remark that *to state—as we shall do—that organization into different units is a universal process in perception, feeling and evaluation does not necessarily imply that 'unity' or 'symmetry' must be consid-*

ered as highly positive values in the field of art.

## II. THE CONCEPT OF 'STRUCTURE'

A widely accepted doctrine on perception in recent psychology is that which states, contrary to the old atomistic schools, that our perceptions are not mere mosaics of single sensations but more or less *organized complexes of different perceptual units*. When we look at six dots ordered in such a way that they form a cross, we do not perceive some chaotic blots of ink, but 'a cross,' that is, a perceptual unit, a perfect Gestalt. It is the merit of the Gestalt psychologists (Wertheimer, Köhler, Koffka) to have brought this fact to light in the field of psychology. As Wertheimer has stated: "The fundamental formula of Gestalt theory might be expressed in this way: There are wholes the behaviour of which is not determined by that of their individual elements, but where the part-processes are themselves determined by the intrinsic nature of the whole" (54, p. 2).

But what are the determining factors in the segregation, coherence and interpretation of these units? This question has received many different answers. Orthodox Gestalt psychologists have emphasized the importance of what might be called 'general objective organizing factors.' Though explicitly granting the influence of past experience, Wertheimer has stressed the importance of these objective forces in several laws regarding the influence of nearness, similarity, sequence, closure and other similar factors (55). Other authors, on the contrary, have emphasized the empirical aspects of perceptual organization. Siao-Sung Djang has carried out an interesting experiment on the influence of past perceptions in actual recognition of objects (16). Cosetti (14) and Galli (19) testing the

doctrines of their teacher Gemelli, have convincingly proved the importance of conceptual meaning in perception. And Zangwill (57) has attempted to show how previous attitude modifies our interpretations of perceived objects. Leaving the detailed discussion of these topics to those who are more directly concerned with this problem, let us take for granted that these and perhaps other factors are determinants of perceptual organization in different proportions; and let us insist upon the general conception of perception as a tissue of structural complexes.

We must keep in mind, however, that experiments in Gestalt psychology have on the whole been performed on very elementary figures which are perfect samples of coherent and well segregated 'structures' (simple geometrical patterns, simplified figures, rough schemes). Neither in the reality of daily life nor in the field of Fine Arts are we usually dealing with such simple structures. If we are to apply configurational concepts to Aesthetics, therefore, *it is very important to enlarge this concept of 'structure' enough to be able to apply it to aesthetic perception*.

The first point we need to make in this connection is the possibility, explicitly granted by Köhler (30, p. 182) and Gemelli (21), *of different degrees of structural complexity* (9). In music a single melodic phrase is usually perceived as a perfect structure; but a set of variations on a given theme, though a more complex entity, may also be considered a perceptual structure. Indeed, it is one of the main purposes of this article to show that a perceived piece of art (whether it be a piece of music, a painting, a poem or a dance) is always a total structure composed of several partial 'structures' which we may analyze into more elementary ones until we reach the simplest perceptual units.

A second important remark in this



connection is the fact, already suggested by Köhler (31, p. 389), that *not all 'structures' have the same degree of coherence and segregation* from their environments. A part of a dance may so gracefully slide into another that no beholder is able to define a boundary between the two. On the other hand, it is also self-evident that the different elements of a given whole may show a higher or lower degree of internal coherence. What is called in painting 'a group,' may be a rather loose aggregate of figures or a compact whole; and even when we are dealing with a perfectly closed set of figures, it is highly improbable that we may discover in the whole group the same degree of perceptual unity that we find easily in each of the different constituent figures.

A third important remark concerning aesthetic perception involves *recognition of the influence of attention*. Kraskowski in 1913 and Chapman in 1935 (12) has experimentally demonstrated the old psychological tenet which states that the more we concentrate our attention upon a certain subject and the more clearly we perceive it, the less we are able to take notice of the surrounding ones. This statement does hold not only for a group of different objects simultaneously, but also for a series of objects successively perceived: the problem of fluctuations of attention is not a new question in Psychology. Let us substitute now for the word 'object' the more comprehensive concept of 'structure,' and we will be acquainted with another fact we must not forget when we try to describe the perception of a piece of art: the fact that not all the structures which constitute it (either the larger or the smaller ones) can present the same degree of clearness.

With this enlarged concept of 'structure' in mind, let us try now to describe the different principal kinds of percep-

tion and enjoyment that we may find in Aesthetics.

### III. THE PERCEPTION OF ART

1. *In music and poetry*. On the basis of some experiments already performed (though further experimentation is wanting in order to prove its truth) we may formulate the following hypothetical principle regarding perception in music and poetry: Were it possible by an artificial device to get a perfect record of the processes occurring at any one moment in a person listening to a musical composition or a poem, we would find in his eardrum a circumscribed set of vibrations corresponding to a relatively isolated sound; yet in his mind we would not be able to discover such a thing as an isolated sound, but rather *a highly organized tissue formed by the traces of the preceding sounds ordered in multiple structures of increasing complexity and of different degrees of clearness, coherence and segregation in which the new sound is inserted as a new part of the dynamic whole (principle of the organic presence of the past)*. This principle has already been applied by Gestalt psychologists to explain the organization of *very simple melodies* (28, pp. 431-452). But what we intend to do here is to emphasize the *possible enhancement of its range of application*.

Herman Reichenbach, in a suggestive article published in the *Journal of Musicology* (43), considers musical compositions (a movement, a nocturne, a symphony or an opera) as true *Gestalten*. He argues that these musical entities fulfill the four conditions required for perfect *Gestalten* by the historical founder of the Gestalt theory, Christian von Ehrenfels. The first of these conditions may unquestionably be found in almost all kinds of musical compositions. Reichenbach writes: "Suppose we distribute the structural moments of a configuration among as

many individuals as there are moments. If the sum of the experience of all individuals is the same as the experience of one individual confronted with the entire configuration, the latter is a purely mechanical link. If the sum is smaller, the configuration has the quality of a Gestalt. Is this condition fulfilled by a form of music? Suppose we distribute its structural moments (themes, cadences, sequences, etc.) among as many men. One or another will certainly enjoy the beautiful melody he gets, but all together they will have much less enjoyment than a single person listening to the whole piece" (43, p. 65).

The other conditions Reichenbach tries to find in every piece of music are the following ones: (a) "Since the essential quality of a Gestalt is not caused by the quality of its structural moments, but by something between these moments, it does not necessarily change if its structural moments are changed. This law is called the law of transposition, from the example of melody. . . . What about transposition of a musical form? . . . Suppose we change the principal subject of a symphony by substituting another first theme of proper style, and change the transition, second subject and coda correspondingly. What is the result? It remains the same kind of composition but we obtain another Beethoven symphony, another Mozart sonata. This interpretation involves the idea that every musical creation is the transposition of another one or that certain compositions are variations of one certain ideal of form: the classical sonata, or the nocturne, or others, . . ." (43, pp. 65-6). (b) "It is a necessary feature of a true Gestalt that its entity appears to us easily and immediately and by no means results from a combination of our conception of its parts. . . . With regard to an entire form in music, we can say that we have an image of

the first movement of Beethoven's Ninth Symphony, even if we are not aware of its different themes . . ." (43, p. 66). (c) "It is much easier to remember a Gestalt than to recall its parts. . . . I remember Franck's D Minor Symphony although just now I cannot recall the chief themes. I would recognize the symphony instantly. . . . If our memory fails us during a performance, it is much easier to play the entire composition again than to start somewhere in the middle . . ." (43, p. 67).

Concerning these latter conditions we cannot agree with Reichenbach's theory. These conditions are easily detected in certain musical structures, like a 'motive' in a symphony; but when Reichenbach tries to apply these rules to highly complex configurations, his reasoning does not appear so sound and convincing. We agree, then, to Reichenbach's theory when he states that musical pieces are real *Gestalten*; we insist, however, on the fact that in order to prove this statement we do not need the perfect fulfillment of all the conditions Ehrenfels required, since the first one is quite sufficient. Musical *Gestalten*, having a high degree of coherence and separateness, are of course very good samples of Ehrenfels' conditions; but we have remarked before that not all structures have the same degree of coherence and separateness.

There are therefore in music many possible kinds of perceptual structures: a chord, a musical phrase, a set of musical phrases, a 'section' of a movement or a piece, a movement, a song, a symphony, an opera. And according to the general principle we stated before, we may say that for musically trained people all the rich tissue constituted by the past played structures of a piece is present in the mind at every note. For instance, while hearing a symphony, I am not only aware every moment of the musical phrase just being played, but

also of the section it is a part of, and of the different parts of the whole movement in which it is played and even of the outstanding features of the whole symphony.

Of course, *not all structures are perceived and kept in mind with the same degree of clearness and stability*. The nature of the perception depends, first, on our own ability and training in music; on the different degrees of attention with which we listen to the different structures, on the mutual relations we discover among them, and on many other factors such as the degree of coherence and separateness.

We should add that the different kinds of structures to be found in music may be divided into two great categories, that of *simultaneity* and that of *succession*. The chords belong to the former, and the melodies to the latter. Notes played successively are joined together into structures of succession (being the simplest form of musical phrase). But notes heard at once either may be joined together into a chord or may be perceived discreetly as separated elements belonging to different musical phrases simultaneously played. Why are notes played at once within a piece of music sometimes perceived as a chord and sometimes not? We think this is a problem that must be studied and explained from the point of view of the Gestalt school, since objective factors of perception are probably the only cause of this fact. 'A set of notes played at once' is therefore a wrong definition of a chord. It may be rather defined as 'a Gestalt of simultaneous notes.' With reference to these problems, we may recall here a very interesting experiment performed recently by Gardner and Pickford, who have proved that a dissonant chord played in different musical passages may be unpleasant or pleasant according to its context (20).

It is strange that the obvious importance of the ability to perceive music as a tissue of structures has been so often underestimated. In his famous 'Measures of Musical Talent' Carl E. Seashore (33, 46) suggested a system of testing the musical ability of a given subject by investigating his sense of pitch, intensity, time, consonance, tonal memory and rhythm. These are indeed excellent tools for the investigation of certain *sine qua non* requirements which must be fulfilled by everyone who intends to learn to play or at least interpret music in a satisfactory manner; but musical talent is something more than these elementary abilities. It implies a special ability to perceive the organic structure of musical pieces.

In *poetry* we have two different levels of organization; that of meaning and that of sounds (24).

When the last word of a certain stanza in a poem is pronounced, we have in mind not only this elemental structure which is the word itself, but also the whole verse in which it is inserted as a necessary rhythmical element, and to a certain extent the whole stanza with its different verses, and even though in a less definite way, the preceding stanzas with their outstanding verses. It is just this conception of the organic presence of the past sounds that explains perfectly *the phenomena of rhythm and rhyme*. They are simple cases of what orthodox Gestalt psychologists have called the tendency to 'closure' and 'good continuation.' Abercrombie gives them a somewhat similar treatment when he speaks of 'the persistence of a pattern in mind' in both rhyme and rhythm (1, 2).

But in poetry we have, simultaneously, a non-perceptual *organization of ideas*. Miss M. R. Harrower has demonstrated (22) in a series of experiments on jokes that in the realm of

thought we may find the same structural organization that has been well studied in the realm of perception. That means that our ideas and our thoughts are never isolated entities passing through our minds like trains on a rail; they are rather like dynamic mental astronomic systems where ideas and thoughts are placed in different positions and attracted to each other by different forces. In this connection we may also apply our general principle of the organic presence of the past. When we grasp the idea suggested by a word or phrase while hearing a poem, we understand its perfect meaning precisely because we already have in our own minds organically arranged all the thoughts contained in the sentences that we have heard before. On this subject Abercombe has also pointed out that "the instrument of poetry is not so much words as language; not so much the separable meaning, however expensive, which can be assigned to this or that word, nor even to this or that phrase, but the continuous organization of this into language, into the process of verbal thought in the broadest sense" (1, p. 97). In this sense we may also interpret the phrase of Hytier when he speaks of the 'melodical succession' of images as the essence of poetical pleasure (26).

In *narrative (epic) poetry*, we may suggest two main different types of mental structures besides many other possible ones: the 'scenes' and the 'characters.' Every 'character' is a definite mental structure to which we relate all the different successive actions performed by this person. 'Scenes' (as 'principal events,' not as mere artificial divisions of a piece of art) are also more or less closed structures which integrate the largest one: the 'plot.' What has been said of narrative poems may also be applied to the *theatre*; but we must grant that we are then dealing

with a much more complex form of art, which has many characteristics of its own, and partakes of other types of art.

We cannot finish this brief set of suggestions on poetry without mentioning the name of George Santayana. His definitions of 'form,' 'character' and 'plot' are not very far from the psychological considerations expounded above (44).

2. In *painting, sculpture and architecture*. In spite of the static character of the work of art in painting, the action of looking at a picture is always a *highly dynamic process* (6, 7). In a recent book, whose main principles are closely related to the ideas we set forth in this article, Morris has emphasized the fact that "in appreciation, all arts are temporal. A painting is not taken in at a glance: its rhythm, balance, its dynamics, are not understood momentarily" (36, p. 28). G. T. Buswell, in his extremely interesting book (6), has given publicity to some experiments performed at the University of Chicago whose main purpose was to investigate by a special photographic technique the trajectory of eye fixation points when an observer is looking at a picture. From the various records obtained it is plainly evident that our sight goes jumping through certain fixation points distributed all over the picture and staying on each one only for a very brief period of time, varying from 3/30ths to 20/30ths of a second.

On the other hand, other experiments on attention (12) and perception (19) enable us to assume that at each moment we can see *with perfect clearness* only a certain structure or group of structures, the rest remaining within the field of vision as objects that we perceive more or less confusedly. That means that mere actual sensations are never sufficient to allow us a perfect contemplation of a painting, since it is impossible to look at all its different



parts at once with the same degree of attention. And it is precisely the *organic presence of the past*—as it is in music and poetry, although in a somewhat different fashion—which helps us in the appreciation of painting. When, after looking successively at the various structures or figures of a painting, attention is concentrated upon a single one of these, the observer is aware not merely of the selected structure, but also of (a) the rest of the picture, as a tissue of different structures or figures, perceived in varying degrees of clearness; (b) the mental representation of those particular structures or figures to which attention had previously been paid.

The necessity of being mentally acquainted with the main structures of the painting in order to have a more adequate contemplation of each of its different parts is perhaps involved when Buswell concludes from his experiments: "Two general patterns of perception are apparent in the records. One of these consists of a general survey in which the eye moves with a series of relatively short pauses over the main portions of the picture. A second type of pattern was observed in which series of fixation, usually longer in duration, are concentrated over small areas of the picture evidencing detailed examination of those sections. While many exceptions occur, it is apparent that the survey type of perception generally characterizes the first part of the examination of a picture, whereas the more detailed study, when it occurs, usually appears later" (6, p. 142).

A similar conclusion has been reached by Brighthouse after no less interesting experiments in which a picture was successively exposed on a screen in a series of presentations each one lasting 0.26 sec. and separated one from another by brief intervals of time in which the observers had to describe what they

had seen in the picture. Brighthouse proposes the distinction of three 'apperceptive stages': (1) attention to general masses, (2) concentration upon small areas, and (3) observations of relations between previously observed features. In all these stages and especially in the last two what we have called the *organic presence of the past* probably plays an important role.

Summarizing all we have said in the preceding paragraphs, we may state as a general principle for pictorial perception that at each moment, *when we are looking at a picture: (a) we are concentrating our attention on a given structure; (b) but we are also aware of the remaining structure of the painting (10), either by a diffused perception of them, or by the mental traces of our more or less accurate previous observations of them.* It is in this sense that we may paradoxically say that a picture is never entirely before our eyes: a perfectly detailed vision of a painting is always a construction of the mind. Nevertheless, this distinction of levels in visual perception is, indeed, a theoretical one. It is by no means an immediate datum of internal experience, but a hypothetical inference we draw from the analysis of the facts of visual perception. What we have in mind as an immediate content of consciousness when looking at a painting is the perception of a unitary whole whose various constituent structures have been successively revealed.

We might ask which are the different principal structures we may perceive in a painting. This question needs a different answer for each painting and perhaps for each beholder. We may assume, however, that when we are contemplating a picture—abstract painting excepted—we usually frame perceptual units according to objects which resemble those in real life (a village, a street, a group of men, a woman, a

hand, a forest, a tree, a flower, a leaf, etc.).

The process of perceiving a *sculpture* is almost the same as that already described for painting. In *architecture* we have also a similar process, but we must remark: (a) that here the impossibility of grasping a whole at once with all its details is more apparent than in painting; (b) that perhaps in architecture 'general objective organizing factors' (law of *Prägnanz*) are much more important than in painting.

3. *In the dance.* If dance is considered as a visual art, it might be defined as an organic succession of *visual structures of movement*. The perception of motion, as that of melody, is always a matter of structural organization. Gestalt psychologists have exhaustively studied this property in single and simple figures of motion (28, pp. 280-304; 30, pp. 148-186; 49). But we may apply this sound conception to dance in the same way as we did in music. At each moment of a dance, what we have in mind is not the mere instantaneous aspect of the dancer (as in a snap-shot photograph) but the various phases and subphases of that dance. This conception of dance as a unity of motion containing subunities within it has been suggested in two of the most recent works on the subject: *Introduction to Dance* (35) and *Dance: A Creative Art Experience* (23). The latter states: "... a single movement is potentially expressive in itself, but unless placed in juxtaposition to other movements that form a larger expressive unit or phrase, it is not significantly expressive,—just as a single note cannot, by itself, be a melody" (23, p. 141).

But dance is not always mainly a visual art. That is perfectly true for beholders, but not for dancers themselves. For them, dance is rather a kinaesthetic art. Although not quite so

well studied as in other perceptual fields, it is evident that the internal perception of our motions present also a structural organization (15). The first thing we need to do in order to learn a given movement is to grasp its *kinaesthetic organization* clearly, that is, to perceive it as a whole structure within which we may perhaps find several kinaesthetic substructures. We may thus assert that perception of dance for the dancer himself is a matter of kinaesthetic organization.

But that is not all we must consider in dance. Serge Lifar (39) has claimed that dance does not necessarily require the accompaniment of music. In his famous Ballet d'Icare he has attempted to create a non-musical dance; and Martha Graham in her well known dances has been trying also to stress the non-musical factors of this art. Nevertheless, this is probably a problem of rating (to what extent pure dance may be a self-sustaining art), and whenever we find dance intermingled with music—whatever the rate may be—we must acknowledge that we are not then dealing with the usual one-sense organization, but with a very complicated form of mental activity, not yet well studied by psychologists, where structures belonging to different sensorial levels of organization are also joined together into *bi-sensorial structures*.

#### IV. THE SUBJECTIVE PROCESS OF FEELING AND THE OBJECTIVE JUDGMENT OF VALUE

1. *Feeling as a process.* In an experiment performed in 1924 in the Psychological Laboratory of Cornell University to show the 'palpability' of affective qualities (37), John Paul Nafe presented to eight psychologically trained observers several visual, auditory, olfactory, gustatory and 'tactual' (strictly tactual, thermal and painful)

stimuli. Among some other conclusions, irrelevant to our purpose, Nafe summarized the observer's descriptions of pleasure and displeasure as follows: "P, in quality, is a bright pressure or a quality lying between bright pressure and tickle. It is described as bright, sparkling, brilliant, active, like effervescence, like tickle, dancing, shimmering, like points of light pressure, mild, misty, yielding, buoyant, vaporous, diffuse, light, airy, thin, fluffy, ethereal, smooth, soft, oily, welling, spreading, like the pressure component in warmth, like goose flesh, fur and glass, like muscle pressure in quality (not in density or localization) but brighter, like expansiveness of the body. The quality of U is that of dull pressure or, according to some Os, of a pressure between dull pressure and neutral pressure or between dull pressure and strain. It is described as dull, drab, dead, rigid, less lively, somber, inert, stiff, gloomy, more dense, heavy, sinking, leaden, thick, cold, hard, rough, harsh, grating, insistent, condensed, like bodily contraction, like neutral pressure but duller, like dull strain" (37, p. 517).

In many of these attributes it is easy to see that *we are concerned with a psychological phenomenon which is in the nature of a process*. This feature is even more apparent in some observers' statements classified by Nafe as 'Affective time relations.' We find there such phrases as the following ones (describing the affective phenomenon felt before different stimuli): "Quite gradually but rather rapidly the affection came on top" (obs. H). "The striking thing is that it grows and wanes rapidly. It has a definite course in time as much as a movement has. It disappears like a dissolving view" (obs. B). "At first sensory (experience), then pleasure, then displeasure, and the displeasure was gone before the sensory hang-over was gone" (obs. N). "Both pleasure

and displeasure come in after the sensory experience and go out with it" (obs. By).

This feeling as a process is also undoubtedly involved in old comparisons between feeling and music (42).

2. *Judgments of value*. In the field of feeling, however, these 'process phenomena' are not the only phenomena to be found. We may detect likewise a *phenomena of a judgmental nature*. In a book on general psychology published in 1925 by H. A. Carr (11) and in different articles by Peters published later in several American journals (41, cf. also 25), pleasantness and unpleasantness have been considered as mere attributes that we ascribe to objects or situations. Notwithstanding the unity of Peters' and Carr's doctrine, we may distinguish in it three principal postulates: (a) feeling is a phenomenon of the nature of judgment, (b) this judgment is made by the individual in the light of his knowledge of his own normal reaction tendency toward a given object, and (c) reactions may become positive or negative by alteration of the spacial relation between object and individual, of the temporal aspect of the experience, or of the intensity of its effects. Leaving aside the last two postulates as highly controversial, let us insist on the possibility of mental contents of a judgmental nature.

George Santayana in his most famous work on Aesthetics has defined beauty as "an emotional element, a pleasure of ours, which nevertheless we regard as a quality of things" (44, pp. 47-8). It seems to us that this phenomenon of 'objectification' is just what characterizes immediately our aesthetic judgments. When we explicitly or implicitly judge a thing as beautiful or ugly, we do not have in our awareness as an immediate and outstanding content the notion of 'our normal reaction tendencies toward it.' It may be that this

reference to our behavior hides in the background of our consciousness, but beauty and ugliness are more generally conceived as 'the ability of an object to arise in the beholder certain pleasant or unpleasant feelings.' This is, however, a problem which may be definitely solved only by means of an extensive experimental research.

In this connection a certain relation between affective judgments and some well studied phenomena of perception may profitably be suggested. We refer to what has been called the 'phenomenal constancy tendency' or the 'phenomenal regression to the real object.' It has been satisfactorily demonstrated that when we attribute a certain color, or form, or size to an object, though certain factors as shift in illumination, or point of view, or distance, tend to make them appear in a very different way, not only do we continue 'judging' them as unchanged in their objective properties, but even in our perception we are acted upon by a force which tends to diminish the amount of change in the stimulus field (50). Robert H. Thouless has pointed out that the properties of a given stimulus in which phenomenal regression occurs are just "those characters which we regard for obvious practical reasons, as belonging to objects. Thus, the whiteness of a paper is regarded as a property of the paper but not its luminosity which depends also on the intensity of the illumination; the length of a rod as measured by a ruler is regarded as a property of the rod, but not its angular dimension as determined by theodolite, since this would depend on the rod's distance and inclination" (50, p. 306). A similar phenomenon occurs in aesthetic experience. When we feel pleasantness or unpleasantness over the contemplation of a piece of art, we tend spontaneously to analyze our feelings and to attribute to the object as a cause that part of our

feelings which we conceive as depending on its intrinsic properties and not on a personal and transient state of our mind. When we are conscious that an object annoys us because we are tired (if we are not conscious it is not the same), we do not conceive unpleasantness as arising from the qualities of the object, but from a perfectly subjective cause; in the same way we do not think that an object has become smaller when we have a smaller image of it in our retina because we have receded from it.

The judgment of value represents thus in aesthetics, at least in most cases, a conscious effort to isolate in our pleasant or unpleasant states that part which 'must' be attributed to intrinsic properties of the object. This recognition of a strong bias towards objectification in aesthetic judgment does not, however, argue either for or against the philosophic conception of value as ontologically objective properties of the things.

3. *The difference between judgment of value and the process of feeling.* The existence of two definite kinds of phenomena in the realm of Aesthetics may be clearly observed wherever a process of pleasantness and a positive judgment do not coincide, or a process of unpleasantness does not occur at once with a negative judgment.

Although scientific experimental evidence is meager on this subject, we may consider some well-known facts of daily life. No matter how much a person may like a piece of music, if it is repeatedly heard a hundred times he will not be able to have in the last hearings the same pleasant feelings he had the first time. Nevertheless this person will probably not change his former judgment of the value of the piece.

Sometimes (the Austrian philosopher, Müller Freienfels [47], has pointed out this fact) we experience a certain feeling (for instance, one of pleasure while



hearing a popular song which brings up certain special remembrances), and yet we recognize—in a more or less explicit judgment—that this song does not deserve, on the score of its formal properties, so enthusiastic a reception.

Another philosopher, Max Scheler (47), has also stressed the constancy of our judgments of value against the great changeability which characterizes feeling processes. This relative stability of our judgment of value has been shown by E. J. Calahan in his experiments on the 'consistency of aesthetic judgment' (8). The experimenter recorded the preferences of a large number of students concerning 63 pairs of pictures, and the reason for these preferences. A year later these subjects were invited to judge again the same pictures and it was found that the list of preferences, while some exceptions occurred, was pretty consistent.

This distinction between the process of feeling and judgments of value may also be supported by an experiment performed by Max Schoen and Esther L. Gatewood (45) in which evaluation of the quality of several pieces of music by individuals was correlated with the degree of enjoyment derived from them. Although the authors intended to demonstrate that "judgment of quality of the music is in direct proportion to the intensity of the pleasure" (45, p. 181), a careful analysis of their figures reveals clearly the fact that judgments of value and enjoyment, though strongly related one to another, are not the same thing. The figures were these:

Judgment of quality	Degree of enjoyment				
	00	0	2	4	6
Very good..	2	1	6	18	70
Fair.....	5	2	4	4	
Poor.....	2				
Very poor..	3				

It is clear that 10 pieces were judged 'Good' or 'Fair' though no enjoyment was derived from them at that moment. Even granting, as the authors suppose, that this anomaly was due to deficiencies in the rendition, it is plainly evident that 'judgments of value' are mental contents perfectly separable from 'processes of feeling.'

4. *The foundation of value.* The relation between value and feeling, at least within the field of Aesthetics—in Ethics it is perhaps much less doubtful—is still a matter of debate (13, 40).

On purely theoretical grounds, Johannes E. Heyde considered feeling processes necessary as a foundation for all kinds of value. Müller Freienfels, on the other hand, stressed the importance of 'tradition' in the appreciation of value, and Max Scheler related the cognition of value to certain special ability of the mind not necessarily connected to the process of feeling (47).

In one of the few psychological experiments ever conducted on that subject G. E. Störing (48) took notice of two different types of evaluation: "the emotional ones, arising out of immediate experience, and those given on the basis of a mere knowledge of value." Most judgments were a mixture of both types.

5. *A practical consequence.* A practical application of this distinction between the process of feeling and the judgment of value is the necessity of explicitly investigating, in *preferential experiments of Empirical Aesthetics when a dissociation of both phenomena is suspected*, whether the observer is stating his objective valuation of the test material or is summarizing his own subjective and transient process of feeling. In a more radical way, this remark was already made by M. Yokoyama when, after a careful analysis of a previous experiment, he concluded that "his results indicate that the method of

paired comparisons can no longer be taken as a typical laboratory setting for affections of process-nature" (56, p. 369).

#### V. FEELING IN THE CONTEMPLATION OF ART

The research in the pleasantness or unpleasantness of very simple elements of perception has been one of the favorite tasks of atomistic aestheticians. Interesting as they are for psychology, it is pretty dubious whether they have a real importance in Aesthetics. I may indeed assert that I like the sound of C more than the sound of F when I am urged to judge both in isolation, but this means very little in the right understanding of my appreciation of art. It is a generally accepted fact that very simple perceptual elements are never able to arouse truly intense feelings.

Beebe-Center, in his book on pleasantness and unpleasantness has summarized many decisive experiments on feeling, pointing out the fact that "*hedonic tone may characterize—may be carried by—any conscious configuration whatever*" (5, pp. 399-400). Every sound, every note and every color may be a fit material for a piece of art; but 'aesthetic enjoyment' in a narrow sense does not begin until these notes have been perceived as a 'musical phrase'; these colors as a 'figure' or a part of a 'figure,' and these articulate sounds as a sentence (or at least as a pair of contrasting words).

But Beebe-Center's statement may be also applied to higher configurations, and we may attempt to make a special development of it on the basis of our previous analysis of aesthetic perceptual experience. We have emphasized in our previous considerations on perception how at each moment we have in mind much more than we have received through our senses and how the

'organic presence of the past' plays a prominent role in perception. Let us go now a step farther and assert that the '*feeling*' that a given structure may arouse in us is not a mere consequence of its internal properties, but depends also, to a great extent, upon the feelings aroused by the contemplation of other structures, simultaneous or previously perceived, and especially on the right or wrong insertion of the structure actually perceived into the organic tissue of the other ones.

Let us explain this abstract principle by means of an example taken from the field of poetry. "Know not the heart" is a collection of words that is not able to arouse any aesthetic emotion in anyone. But if we hear this little phrase as a part of a larger one: "They draw but what they see, know not the heart" then these formerly indifferent words become tinged with hedonic tone. We like them just because they are perceived as a part of a verse whose iambic framework has been already suggested by the part of the poetic phrase heard before. And this enjoyment still increases when we have in mind, while listening to these few words, not only the first part of the same verse but also the preceding one:

*Yet eyes this cunning want to grace  
their art  
They draw but what they see, know not  
the heart.*

Now the prosodical structure of the stanza is harmoniously closed, and a meaning structure has also been completed. . . .

This tentative description of aesthetic enjoyment may account for some facts observed in several psychological experiments. Let us recall, for instance, an experiment conducted by M. F. Washburn, M. S. Child and T. M. Abel in studying the effect of immediate repetition on the pleasantness of music

(52). It was found that five consecutive performances in popular music (fox-trot, one-step) tended to lower pleasantness, while in classical music (Schubert, Beethoven, Tschaikowsky, Haydn, Wolf Ferrari, Nicolai) repetition tended to raise it. The introspective reports made by musically trained observers revealed as main determining factors for the increase a better understanding of classical compositions. It may be easily explained if we assume that classical pieces have a richer structural organization that is not easily apprehended if heard only once, and that the increase of pleasantness is due merely to the possibility of inserting each passing group of notes in larger configurational organizations, better and better apprehended. By this analysis of the aesthetic process of feeling we do not mean that the *structure we perceive last in a piece of art is best*. By no means! Even the first structure may be the most pleasant in any work of art. What we would have in that case is a 'small structure' so full of formal and expressive beauty, so rich in internal harmony and external suggestions that it does not require anything else to be enjoyed with a very high degree of pleasantness. That is, for instance, the case with the famous Spanish sonnet where the author, in trying to describe a large-nosed person, begins the first stanza with this unsurpassable sentence: "There once was a man to a nose attached." What we mean here is only that at every moment in the perception of a piece of art you must have in mind, influencing your actual enjoyment, willy-nilly, the organized traces of the previously perceived structures.

#### VI. JUDGMENTS OF VALUE IN THE CONTEMPLATION OF ART

Though feeling as a process and comprehension of value as a judgment have

been carefully distinguished above, we must yet point out that *development of a value judgment is based on our perception of structures in much the same way as is the development of feeling*. When we are judging a certain partial structure of a piece of art—and all structures in art are partial, except to a certain extent the whole piece itself, *we judge it from a two-fold point of view: (1) that of its internal content; (2) that of its insertion in a higher organization*. Let us call the former the absolute or internal value of a given structure, and the latter the relative or external one.

Concerning the internal value of a judged 'structure'—either the whole piece or any part whatever of it—we must recall the famous Gestalt apothegm that "*A whole is something else than the sum of its parts.*" Good experimental evidence on this subject has been already supplied by M. F. Washburn, D. Haight and J. Regensburg (53). In an experiment performed to investigate the pleasantness of color combination compared to that of the colors seen singly, by means of the method of single stimuli, it was found that the aesthetic value of a given pair of colors when judged as a whole was sometimes different from that which would be expected on the basis of the summation value of its single components. The authors summarized their results in the following words: "In the entire series of our experiments, in which 32 combinations were used, with over a hundred observers judging each combination, there were 861 cases where the component colors, when judged singly, were both found agreeable. In 263 of these cases, or 30.5%, the combinations of these colors were found positively disagreeable. There were 465 cases where the component colors judged separately, were both found disagreeable; of these 72, or

15.4%, were agreeable combinations" (53, p. 146).

But the value of a given structure depends also, in a very important manner, on the right or wrong insertion of it in a larger tissue of structures and on the hedonic value of this higher organization in which the given structure is inserted (29). This is what has been called above 'external or relative value' of structures. It is a well known fact of common experience that a certain joke—withstanding its internal aesthetic value as a wonderful expression of humor—is ranged mentally in a very low rank of appreciation when an inexperienced author places it wrongly immediately before the climax of a terribly sad tragedy. And the more we like the whole tragedy, the more we hate that disturbing unfortunate joke. Many other examples may be offered of this essential condition in the judgment of partial structures.

But though these two sides of value judgment may be carefully separated, we are able also to fuse them into a perfectly unitary judgment and to speak then without any circumscribing attribute of 'the value of a structure.' Our judgments of value, are, therefore, a high mental process of synthesis in which are often included even contradictory judgmental aspects. In a more complex philosophical theory H. H. Dubbs (17) has rightly defined 'value' as 'a balance of satisfactions and dissatisfactions.'

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# LAMARCKIAN-DARWINIAN REORIENTATION

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Psychologists nowadays seem quite generally to take it for granted that Lamarckianism is dead and buried. Yet there is an old superstition that, if the dead aren't buried properly, their ghosts may linger on to haunt the living. That is apparently the case with Lamarckianism, the theory that the adaptations one achieves by exercise or learning are hereditarily transmitted to his offspring.

In spite of its supposed disproof, this earlier hypothesis of environmental initiative in development is still very prevalent, perhaps because it shares the present popularity of environmentalism generally.<sup>1</sup> A survey recently made by the writer in classes in introductory psychology indicates that, regardless of much contradiction, it is still the only theory of evolutionary development which most of the students understand and accept. Even in the literature of the life sciences, such as anthropology, genetics, paleontology, psychology, sociology, and taxonomy, conjectures are still quite frequent that something like hereditary transmission of acquired adaptations must somehow, in some way, be accomplished, not only for plants and animals, but for humans also. Implicitly involved in the Lamarckian-Darwinian dispute are most antithetic theories of human improvement, such as education versus maturation, culture versus 'blood,' eugenics versus eugenics. For some writers, delimitation of the science of psychology

depends on the validity of a distinction between phylogenetic (germinal) and ontogenetic (empirical) developments within the individual. Our apology for presuming to exhume Lamarckianism, therefore, is that its ghost still roams the earth, restless and uneasy, begging for a quietus.

Perhaps the chief reason that Lamarckian theorizing still persists is a lurking, un verbalized suspicion that the real issues of Lamarckianism versus Darwinianism (or more properly, Weismannism) have never been realistically conceived or stated, and, therefore, have not really been finally resolved. This tacit reservation is perhaps reflected in the fact that the Lamarckian hypothesis is still presented in most textbooks of the various branches of both biology and psychology, and frequently of anthropology and sociology also, even though the characteristic conclusion is usually added that the theory is false "because the acquired characters do not persist in the absence of the special environment that produced them."

Although in such discussions it is sometimes recognized that acquired characters may occasionally continue in the second or third generation after removal from the environment that induced them, as some Lamarckian experiments have indicated, this effect is usually explained as a result of partial persistence in some form or other of this environment, perhaps as stored food and minerals, or as enzymes, toxins, etc., which may even be carried over to succeeding generations from the body fluids of the parents, especially from the mother. Since these effects generally 'wear off' sooner or later after restoration to the original

<sup>1</sup> Demonstration of inheritance of acquired characters has recently been claimed by T. D. Lysenko as a result of extensive experiments carried on in Russia. See T. D. Lysenko, *Heredity and its variability* (Trans. from the Russian by Th. Dobzhansky). New York: King's Crown Press, Columbia University, 1946.

environment, it is, therefore, concluded that they cannot be hereditary.

# I. THE BASIC ISSUE, PERMANENCE VERSUS CHANGE

It should be pointed out that this negation of inherited acquiredness involves a tacit application of Weismann's famous test of inheritance, that inherited traits will persist in the absence of the environment that first produced them; while, on the other hand, environmental traits are more transitory. A primary purpose of this paper is to show that this Weismannian criterion is inconsistent and equivocal, and should, therefore, be recognized as one of the obsolete dogmas of heredity.<sup>2</sup>

It is the writer's opinion that the basic fallacy in Lamarckianism is not so much that its specific claims are false as that Weismann's method of proving them false is fallacious, and that confusion is the unavoidable consequence. Weismann's test is inextricably involved in the prevalent conceptions, not only of Lamarckianism, but of acquisition and inheritance in general; consequently, all of these concepts must stand or fall together. The Lamarckian ghost is conjured, therefore, not to praise it, but to inter it properly—to pacify it by laying the inconsistent

Weismannian concept beside it. The present purpose is not to defend Lamarckianism, but merely to detach the tangle of genetic misconceptions that has grown up around it. These genetic stereotypes have become so imbedded in the subject matter of the various biological and quasi-biological sciences, such as psychology, that long after their obsolescence should be apparent to everyone, they continue to distort interpretations of various vital issues for which genetic concepts are basic.

While many modern geneticists are probably quite cognizant of the various inconsistencies in their former conceptions of phylogenetic issues, which unfortunately still linger on in the general scientific literature; they are probably so busy with their newer approaches that they prefer to let bygones be bygones as far as the old ones are concerned. In any event, they have not accomplished a great deal in the way of ridding lay people, or even their own biological brethren, from certain earlier genetic fallacies which unhappily still tend to distort our common understanding of life. To some extent, therefore, this paper is intended as a reorientation job for non-geneticists, an attempt on the part of a psychologist to reconstruct the genetic foundations of his own training, and, incidentally perhaps, to loan his remodelling plans to other psychologists also.

Certainly, if psychology is, as we like to claim, one of the biological sciences, psychologists should have as much concern about genetic theories as have the rest of the life scientists, as for instance, taxonomists, embryologists, morphologists, or physiologists. Strangely enough, however, many of us have not accepted our community with other members of the biological family, either by way of exchanging helpful advice and support, or even of passing acquaintance.

<sup>2</sup> In a sense the present paper is a continuation of a previous one, entitled 'The obsolete dogmas of heredity' (14), in which the writer attempted a similar criticism of certain stereotypes which are inherent in our more basic concepts of inheritance and learning. The particular purpose of the previous paper was to show the fallacy of accepting development in the absence of environmental stimulation or learning as a test of the inheritance of any characteristic; this paper exhibits an error which is a special case of the first one: that inheritance of a new characteristic is supposedly demonstrated by the development of it in the absence of the special environmental stimulation that first produced it. Acquaintance with the previous paper may facilitate evaluation of the present one.

## II. THE TYPICAL CRITICISM OF LAMARCKIANISM

As has been suggested, the reason most generally offered in current texts for doubting Lamarckian effects—for denying that changes in creatures which appear when they are transferred to a new environment are ever inherited—is that the new characteristics last only so long as, or at least not much longer than, these creatures continue to live in the new environment, and that they revert back to the old form shortly after they return to the old environment. It is commonly assumed that, if the new characters were really 'hereditary,' they would continue indefinitely in the offspring, even if they were removed from the environment which originally produced the new characters.

This test of the inheritance of a newly developed characteristic, namely, that it must persist in successive generations after they are removed from the environment in which the characteristic was originally developed, was proposed by August Weismann (26) in 1893. Its soundness has apparently seldom been challenged. According to a current text in genetics, "The point at issue is whether . . . the offspring develop this same character in the absence of the same stimulus." While Darwinians and Lamarckians may differ sharply about what the test proves, they have, strangely enough, little disposition to question the validity of it. That it is a fair test and determinative seems to be taken for granted by most biologists and psychologists; at least, it is usually accepted as the final arbiter of all questions of inheritance in most psychological articles and books.

It was evidently so recognized by Paul Kammerer (16), who had produced certain new skin colors and spots in salamanders by rearing them for several generations in surroundings of a different color and humidity, and be-

lieved he had demonstrated inheritance of acquired characteristics. He discovered, however, that the markings slowly returned to their original form when successive generations of the salamanders were restored to their original environment. Some persons believe that his subsequent suicide was caused by his disappointment. It is hard to understand how he could accept such a questionable test of inheritance so unquestioningly.

As has been said, the first purpose of this paper is to show that this test of the self-sufficiency of heredity (or permanency of form in spite of environmental change) which Weismann considered necessary to prove inheritance, is illogical and invalid. The reason this permanency test is invalid is that it sets up an equivocal and impossible standard for determining inheritance, a test that no organic characteristic of any kind is ever able to satisfy; since, as Dobzhansky (4) has recently emphasized, maintenance of the characteristics of any living creature through successive generations is always dependent upon continuance of the later generations in the same environment in which the earlier ones lived. The very fact that the later or new characteristics could be 'acquired' at all proves beyond question that the earlier, presumably inherited, characteristics were really not independent of their environment as they were supposed to be, and could not themselves satisfy the test of inheritance which Weismann applied to the later ones. Since they changed form when the environment first changed, the 'inherited' characteristics were quite obviously not inherently controlled or pre-determined.

## III. EQUIVOCAL INTERPRETATION OF EXPERIMENTS

An example of the confusion that may result from application of Weismann's



criterion are the diverse interpretations of the widely quoted experiment by Schroder (22), in which he transferred the eggs of a beetle, which lived and laid its eggs on a species of smooth-leaved willow, to the leaves of a different downy-leaved willow. After three generations had lived on the new kind of leaf, the following generation was given equal opportunity to choose either kind of leaf. The fact that the new leaf was chosen was offered as evidence of inheritance of acquired characteristics. Detlefsen (3, p. 250) objected to Schroder's conclusion on the ground that he did not establish "whether the modification would persist, or disappear gradually if a reasonable number of additional generations were followed under normal conditions."

The findings of Schroder are typical of Lamarckian results in general; a new characteristic is developed and increased through a few generations by rearing them in a new environment, and persists for a period, perhaps a few generations, after the bearers are returned to the old environment. This is the story of the peculiar markings and structures developed by Kammerer on salamanders, and also possibly of the increased ability of successive generations of rats in running water mazes, reported by William McDougall (18), as well as dozens of other Lamarckian observations. The criticism of these findings by Detlefsen, that the effects are not permanent, is typical also; similar objections to Lamarckian findings can be found in the majority of texts on biology and psychology.

Insofar as they serve to disprove the claims of 'inheritance' (according to Weismann) of characters which are 'acquired' (according to Lamarck), these criticisms are perfectly justified. Continued observations usually do show that the acquired quality does gradually disappear if the creatures are re-

turned to their earlier environment. That is what happened for the new traits developed in amphibia by Kammerer, and, as predicted by Detlefsen, it was found that Schroder's beetles could be readapted to the smooth leaves on which they originally lived, just as easily as he first shifted them to the downy ones. As a matter of fact, the type of beetle he produced experimentally has since been found naturally existent, feeding on the downy leaves, and, if and whenever necessary, both forms probably shift to the alternative type of leaf.

It is perhaps a safe prediction that, if successive generations of McDougall's environmentally-improved strain of rats were given negative practice in maze running, that is, merely allowed to live in idleness as do most white rats, they would lose their supposedly hereditary ability just as fast as they previously gained it. Such a consequence should be expected if the new ability is dependent on a new environment, *i.e.*, on an increase in a stimulating autacoid which is a by-product of maze-learning activity. If reports are correct, such degeneration of McDougall's Lamarckian rats has actually happened, in spite of the fact that the breed has been kept intact.

A major difficulty in evaluating Lamarckian evidence, therefore, or even more, in dealing with the Lamarckian problem at all, is the unavoidable and insuperable difficulty of knowing, on the basis of objective evidence (independently of the experimental bias of the observer) which are inherited and which are the acquired characteristics. Obviously, if Schroder had happened first to discover the other type of beetle, which feeds on the downy leaves, he would naturally have concluded that this beetle was the true and original or 'hereditary' variety. In that case he might very well have directed his ex-

periment in the reverse direction, might have developed the type of beetle which he happened to find first, and, therefore, have claimed that its 'acquired' characteristics had become 'inherited.' As Lindsey (17, p. 420) points out, either habit may be regarded as positive. 'True' or 'hereditary' forms are evidently those which happen to be first observed, just as 'true' love is often love at first sight. It seems that 'finders are keepers,' just as much in identifying hereditary characters as in claiming lost articles.

A similar criticism can be made of most Lamarckian experiments, regardless of whether success is claimed for them or not. It is rather an ironic retribution, however, that if such criticism of Lamarckian claims is carried to its logical conclusion, it is certain in the long run to destroy its own argument. The critic who claims that reversion to original type in the original environment proves that the developed trait is not inherited, must, or at least should find that in the long run his argument backfires on him; it must finally prove that the form which he himself first assumed to be hereditary, is itself not inherited according to his accepted criterion of inheritance, since it also changed form. If a certain degree of automatic stabilization of the immediate environment within the organism (homeostasis) is recognized, it is evident that all characteristics of organisms last just as long as all of the interacting factors, germinal and environmental, remain the same, and no longer. Consequently, if the Lamarckian problem is, as commonly conceived, the problem of inheritance (in the Weismannian sense) of characters which are acquired from the environment (in the Lamarckian sense) it is bound, sooner or later, to reduce to absurdity and become meaningless. Arguments about it,

on this level, are really much ado about nothing.

#### IV. ACTIVE VERSUS PASSIVE ACQUISITION

While the kind of 'acquisition' which is illustrated by Schroder's beetles, that is active or adaptive acquisition, has always been the chief object of concern in genetic thinking, since it is the only kind of variation which can benefit the organism or lead to evolutionary development, still it must be admitted that the environment can and sometimes does achieve another type of change in organisms which is obviously passive, rather than adaptive. When, for instance, a bullet is shot into its tissues, an organism is entirely inactive in the development; the change in the tissues is altogether non-adjustive, at least at the moment; the bullet wound is *inflicted* on the organism rather than *acquired* by it. This kind of acquisition is quite obviously not that which both Lamarckians and Darwinians have typically been concerned about, since they have always been looking for self-directed changes in the organism which may possibly culminate in organic evolution. Strangely enough, however, such passive, non-adaptive change was once made the chief object of attention in one of the oldest and most widely-quoted of all Lamarckian experiments; namely, that in which Weismann (26) cut off the tails of mice for several generations—with negative results in so far as shortening the tails of the offspring is concerned. It is difficult to understand how or why amputation of tails, a wholly imposed effect, could ever have been seriously considered as an example of acquiredness, either by Weismann or those who cite his curious experiment so frequently. Taillessness was inflicted on these creatures, not acquired by them. The mice had no share in the de-tailing; it could just as

well have been accomplished with dead mice as live ones. One might just as well expect clipped hair or nails, or cleanliness, to become 'hereditary.' In the only possible evolutionary sense, changes are acquired only when they are adaptive and potentially beneficial, at least to the lineage, and only when the organism coöperates in creating them. Failure to distinguish active from passive environmental changes has been responsible for much confusion in genetic thinking.

#### V. IS THERE A LAMARCKIAN-DARWINIAN ISSUE?

We must conclude, therefore, that no adaptive biological characteristic (of which tan and calluses on the skin are examples) is ever really 'acquired from the environment' unless the inner organism is prepared to coöperate in the acquisitions; no characteristic is ever 'contributed by heredity' without the help of the special environment in which it appears. Also, no so-called hereditary characteristic is ever permanently maintained in the absence of the special environment in which it first appeared.

When everything is taken into account, therefore, it must be admitted that the theories of Lamarck and Weismann are not essentially different; they really add up to the same total. Each man minimized, however, what the other emphasized. Lamarck recognized the part played by the environment in development, but ignored the indispensable germinal contribution; Weismann centered solely on the germinal influence on growth, but forgot that nothing can ever come from the germ except for the help of the environment. As a matter of fact, Weismann tacitly assumed that every characteristic must, in some way or other, actually exist antecedently in the germ plasm, prior to its phenomenal appearance—a very

appealing but deceptive hypothesis. There is increasing evidence, however, that the environment always contributes form as well as substance to every organism.

If this interpretation of the joint action of heredity and environment in growth is accepted, one must necessarily also accept as a corollary that every organic characteristic, if it appears at all, is as much inherited as acquired. Without the hereditary component, no trait could appear at all; and without the environmental complement, the hereditary potentiality could never be realized in actuality. This statement would, of course, be just as true if the words hereditary and environmental were interchanged. For reasons presented in more detail in previous papers (11, 12, 13, 14), we must conclude that no adaptive characteristic is ever more or less inherited or acquired than any other characteristic.

The answer to the question, Are acquired characteristics inherited?, may, therefore, be either No or Yes, depending on whether the emphasis is on inheritance or acquisition.

From one viewpoint, obviously, 'acquired' characters cannot *become* 'inherited,' as commonly supposed, because they *are* already as much 'inherited' as any character ever can be inherited. The 'acquired' characteristic cannot be transmitted from the part or organ in which it is 'developed by use' and 'imbedded in the germ cells' (as was argued pro and con by Lamarck, Darwin, and Weismann) because it is already as much imbedded in the chromosomes as any characteristic ever can be, else it would not have appeared in the first place.

From a contrasting point of view, however, the answer to the Lamarckian question might be Yes, insofar as any characteristic is ever inherited or acquired, 'acquired' characteristics are in-

herited. The new or developed characteristic is passed along from generation to generation as long as the new immediate environment is maintained, either directly or homeostatically, and that is all that is possible for any characteristic, as the fact of change from the original form demonstrates.

At this point the query of the critic may be, Isn't this a very weak and limited kind of inheritance? The answer must be that weak and limited as it seems to be, this is the only kind of inheritance there is; there is never any other. This limitation should be a constant reference in our thinking about inheritance; failure to keep it in mind incurs constant danger of rational miscarriage. The search for an omnipotent and self-sufficient heredity is a groping in the dark for something that isn't there, a quest for a pot of gold at the foot of a rainbow which has no foot.

On the other hand it is equally futile and deceptive to conceive of environment as the sole or principal source of the so-called environmental characters, or even to try to evaluate the separate contributions of either environment or heredity, any more than for a chemist to attempt a discrete measurement of the contribution of either oxygen or fuel in the production of heat. Chlorine may be substituted for oxygen in the combination, or a different kind of fuel provided; and a difference in heat production may be measured. Such observations, however, are relative only, and provide no indication about the separate or fractional contribution of any of the interacting components, either in the process of combustion or of organic development. In the traditional sense of being altogether the product of either the germ-plasm alone, or of environment alone, no physical or behavioral structure is inherited or acquired. As the writer has argued previously (11, 14), differences only

are hereditary or acquired; characteristics of any kind either physical or mental, are not. If this interpretation is accepted, the problem of the inheritance of acquired characters necessarily becomes meaningless. Experimental evidence favoring such an interpretation of the differential relationship between heredity and environment has previously been presented by Donald O. Vine and the writer (13).

The Lamarckian problem is, therefore, a spurious problem—the illegitimate offspring of an equivocal distinction between inherited and acquired characteristics. While the effects of this false standard are perhaps most obvious in relation to Lamarckianism, it has probably wrought a much greater, even though more subtle, cultural disaster in perverting our conceptions of development generally, in starting mankind off on a wild-goose hunt for 'inherited' and 'acquired' traits. We have always naively assumed that 'inherited' traits are those which are permanent or continuous because of a given germinal lineage, failing to realize that such permanency is just as dependent on environmental as on germinal constancy, and also that traits which are 'acquired' through environmental change are just as much the creations of germ plasm as the 'inherited' ones.

## VI. HOMEOSTASIS AND HEREDITY

For some readers, perhaps, the fact that organic states are apparently sometimes quite stable in spite of temporary changes in their outer environment (as the temperature of the body when the air becomes warmer) may seem to deny the above interpretation that organic constancy depends on environmental constancy. The mere fact of organic constancy, however, is no warrant for concluding that such stabilized or 'hereditary' processes are independent of the environment. Evidence is increas-



ing that organic stability is essentially an aspect of the phenomenon of homeostasis; it results from a sort of buffering of the inner or proximate environment of organisms (temperature, moisture, oxygen, food, minerals, etc.) against rapid changes in the outer environment, like a man in a house.

Homeostasis is not solely a hereditary or even an organic function, since it often involves coöperation between several organisms, as maintenance of the uniform oxygen content of a balanced aquarium, or temperature of decaying manure. Sometimes, moreover, environmental equilibrium is purely a physical process, as the constant temperature of geysers, of a freezing mixture of ice and water, or of sea water. In the organism stabilization reduces finally to the fact that after the outer environment changes, various residues from it still linger on in the inner environment of the organism. This residuum shows strikingly in the acid-alkaline, mineral, and oxygen balance of the blood, and also in organic adjustments to persisting enzymes, autacoids, or toxins and antitoxins, carried over from some previous interaction with an earlier environment, perhaps a noxious one. As Lindsey (17) says, "The major tendency of evolution is towards greater independence of external environment," but it should be realized that such independence is achieved only by incorporating and stabilizing the outer environment in the inner environment.

Although an organism may adjust and establish a new equilibrium after being disturbed by environmental changes, it is really never the same thereafter. A partially new creature is thus evolved. After recovering from a serious illness, or even from ivy poisoning, the whole bodily economy is permanently different. Vaccination may effect lasting immunity, and administration of hormones may permanently change the form of

the body, and also the temperament, even though a new type of equilibrium is finally established. Also, some of these adjustments, as certain immunities to disease, for instance, and probably many other effects as well, are chemically transmitted from the blood stream of the mother to her unborn child along with its food and oxygen. There is an increasing number of reports of findings that certain supposed hereditary effects are really hormone effects instead, or perhaps it would be better to say that such hereditary effects are achieved by means of hormones.

#### VII. THE CHEMICAL THEORY OF DEVELOPMENT

The prevalent theory of the means by which the genes exert their effect on growth is a chemical theory; it holds that each gene starts a unique enzymic chain reaction, which in interaction with other similar chains produces a living organism. Darlington has termed the gene a 'packaged chemical.'

This chemical theory of the gene may perhaps seem strange and incomprehensible to anyone who is accustomed to thinking in terms of the older preformation theory of hereditary transmission which was accepted by Weismann, that "all the characteristics of an organism exist antecedently in the germ cell." It is now generally recognized, however, that the form of the organism does not come solely or even primarily from the genes or chromosomes, but is a coöperative product of all the factors, germinal and environmental, that interact to create it. The idea of antecedent miniatures as the sources of later developments is really a heritage from magic. The form of an organism is derived from the encompassing whole, just as is the form of a crystal, not from any discrete source. Changes in any of the interacting fac-

tors, either germinal or environmental, may differentiate the resultant organism, but none of them can control or determine it. Obviously, since it has been demonstrated that the molecules that constitute the genes are relatively simple in structure, complex bodily form could not possibly come from them. As the writer has argued previously (14), the genes are really *not determiners* of characteristics, but only *differentiators* of them.

A significant indication of recent biochemical research is that genes and virus molecules are very closely related, both chemically and in behavior. They have a very similar and evidently quite definite molecular structure; both reproduce themselves; and both are subject to sudden persistent changes or mutations that breed true. Both occupy the dim mid-region between life and non-life; they are evidently at once both the smallest units of life and also identifiable chemical compounds. Moreover, they are quite similar to certain enzymes and glandular hormones, and it has been demonstrated (23) that their effects are sometimes achieved by a continuing chemical process. Although hormones are typically not self-perpetuating, there is much warrant for conceiving both genes and viruses as self-reproducing or multiplying hormones; since their effects are similarly achieved. Potter (21) proposes that "life began when a small group of enzymes became organized for mutual benefits." It is well known that variations in any of these agencies may influence the growth and form of the organism. It is also conceivable that any of them (but especially viruses) may be transmitted from parents to progeny, not only by way of the stored-up portions in the chromosomes (called genes, resembling seeds, or yeast 'starter'), but perhaps through any and all means of contact between generations. Had-

dow (10) presents evidence that hereditary effects may be transmitted either through the blood fluids of the mother before birth or in her milk after birth, as well as from the cytoplasm of the gametes of both mother and father. Mere contact serves to transmit from one generation to the next the virus that supplies the mosaic characteristics to tobacco plants. Spiegelman and Lindegren (23) have shown that a yeast enzyme may be started by a gene but maintained thereafter chemically, in the absence of the gene, so that a chemical is substituted for the gene in producing later effects.

According to Darlington (2, pp. 167-168), "A virus, injurious to one host, can exist in equilibrium for hundreds of years in another, like the broken Zomerschoon tulip. . . . It is apt to undergo mutation and consequently shows adaption. . . . Unrelated viruses may interact, and even reinforce one another, as nucleur genes do. . . . The virus will have become a part of the host's heredity. . . . Is there then between the infective virus and the inherited plasmagene an ultimate and absolute distinction?" In the case of cancer, "reproductive particles can suddenly appear in the cytoplasm by action either of the mutafacient nucleus or of external carcinogens. . . . A normal and necessary cell particle has become both infectious and hereditary, both a virus and a plasmagene, at one stroke. . . . The high frequency of plasmagene and virus mutations . . . gives an almost Lamarckian color to their adaptation."

Darlington proposes that there are three systems or levels of hereditary transmission: nuclear or chromosomal; corpuscular or plastid; and molecular or hormonal. These systems are mutually interdependent and adaptive. He concludes, "There is a common reservoir from which the new material

of heredity and infection is continually being drawn. The frontiers that exist between the studies of heredity, development and infection are thus technical and arbitrary."

A partial explanation of the 'heredity' of certain families and races, therefore, may be that they are quite universally infected with certain microorganisms or viruses which function as self-maintaining enzymes. Consequently, such peoples have developed certain common immunities or adaptations which persistently influence both their morphology and their behavior. It is recognized that widespread uncinaria infection is a primary cause of the laziness and lankiness of 'hill-billies' in some of our southern states. An extreme of this viewpoint might perhaps conceive a man as a sort of walking federation of virus diseases. The source of such federation, however, still remains to be explained. Also, as A. E. Boycott suggests, "... if one postulates a normal virus occurring in normal cells, one had better call it something other than a virus."

If the transmitted virus or hormone fails, either partially or completely, to multiply and maintain itself, and is not resupplied, regardless of its temporary influence, it must gradually become so diluted that its effect is finally lost especially in subsequent generations. Haddow (10, p. 195) cites evidence that the cytoplasmic factor may be conveyed by the mother's milk, and sometimes "becomes inactive after a number of generations when a chromosomal factor is not present at the same time." This is a possible explanation of the fact that so many Lamarckian effects have 'run out' in the second or third generations. In some instances a special, maintained activity of the organism seems to be necessary in order to re-supply or maintain such agents. Adrenal and thyroid glands tend to

atrophy if a creature is inactive; muscular growth is apparently mediated by the chemical by-products of muscular exercise.

It is possible, therefore, that the improved ability with continuing practice of successive generations of McDougall's rats was promoted by certain similarly accumulated, chemical by-products of learning activity, which in time (since the rats were no longer actively learning) naturally became dissipated. An increase in the supply of thyroxin, for instance, might possibly be responsible for such improvement.

From the traditional point of view, of course, such an autacoidal interpretation of transmission might be considered as alternative or competitive with the Lamarckian explanation proposed by McDougall. However, if it is true that hereditary effects are really achieved by chemical means, then perhaps there is no real conflict after all. It is conceivable that transmission of the 'hereditary' effect in such an instance might be by means of a maintained or multiplying chemical agent, the 'seed' of which is stored in either the cytoplasm or the chromosomes to be transmitted to the progeny during conception. While it is probably unnecessary to postulate chromosomal transmission in the case of McDougall's rats; still it is entirely conceivable that an acquired virus infection might spread throughout the organism and finally, as Darlington proposes, find lodgment in the cytoplasm of the gametes, or even in a chromosome as a gene, and thus be transmitted so as to continue its effect in later generations. It is obvious also that such an effect might with some justification be considered Lamarckian. Its permanence would depend upon maintenance of the whole set-up of interactive factors, however, rather than on the dominance of 'hereditary determiners.'

### VIII. SELECTION, THE POPULAR ALTERNATIVE FOR LAMARCKIANISM

If the traditional conception of the Lamarckian problem, of 'acquired inheritance,' is inconsistent and fruitless, what then? There still remains, of course, the more general problem of organic adaptation, of how life forms and their environments became so intricately adjusted and adapted to each other. As mentioned previously, it is the primary purpose of this paper to try to point out certain ambiguities in interpretation of the supposed problem of inheritance of acquired characters, to exhibit certain fallacies that have perverted thinking about inheritance and acquisition, not to survey or attempt to evaluate alternative explanations of organic evolution. Our attitude towards any particular theory, however, is always colored to some extent by our attitude towards possible alternative theories. In the present instance, moreover, it is worthy of note that the same basic fallacies in reasoning that distort our common interpretation of the Lamarckian theory also misguide our efforts to formulate a realistic alternative theory.

One chief reason for popular rejection of the Lamarckian theory by those not familiar with the findings of modern genetics is the one which frequently determines the rejection of suitors generally; namely, that a more promising candidate is available. And just as it is often true that a preferred suitor is sometimes romantically rated far beyond his real merits, so also is the popular alternative for Lamarckianism. This is the theory of natural selection of germinal variations, the so-called Darwinian theory.

One chief reason for our universal confidence in the variation-selection theory is that it seems to work practically, that plant and animal breeders progressively improve their stocks by

discarding the poorer individuals and selecting for breeding only those which happen to show the desired tendencies, at least in their ancestors. Thus breeders have produced vastly improved flowers, fruits, and grains, as well as many varieties of cattle, horses, and dogs. Darwin proposed that Nature selectively evolves new species in the same manner, by favoring survival of fortunate variants.

There is apparently no end of lay optimism about the possibilities and prospects of such a selective technique of improvement. Yet all geneticists, and also many breeders, now realize that most of this popular confidence in the long-range possibilities of selective breeding is unwarranted, since research has discovered that there are strict limits to the possibilities of such development. It must be admitted that the method of improvement by selection of desired types from among the multitude of common variations that we see everywhere in living things (that is, by choosing from among those variations which geneticists say result from recombination of the available genes in an interbreeding group or population) cannot be continued indefinitely. For some breeding developments, such as iris for instance, such limits have already been practically reached, and breeders realize that further improvement must necessarily be continuously slower and more difficult.

Such restriction of change is exhibited by the classic experiment by Johannsen (15) at the beginning of the century. He found, in brief, that the size of the seed produced by a certain variety of bean plant cannot be increased beyond certain limits by the customary method of planting only the seeds of plants bearing larger beans (provided, of course, that they are reared in the same environment). By self-fertilization of successive generations of plants bearing



the largest beans (for instance) he was able to develop an unvarying or pure type of plant which consistently produced beans of maximum size with minimum variation in size. This 'pure line' was proved to be a limit beyond which it is impossible to change the size of beans (or any other characteristic) by selective breeding or 'sorting' of the common or usual variations. Our pure breeds of plants and animals are essentially such pure lines. It is now realized, however, that breeders have, as a rule, merely improved strains within the existing germinal limits by restricting parenthood to certain preferred combinations of genes which first came about as a result of chance rearrangements of the existing genes or chromosomes. As T. H. Morgan (19, p. 96) says, referring to such sorting of factors, "The process comes to an end as soon as these factors are sorted out." The evidence indicates that, except for occasional 'accidents' to the genes themselves (mutations), similar limits to progressive variation exist for every variable of every population. While the possible permutations and combinations of the available genes are many, they are not limitless. It is as if Nature provided a number of stops by which the tonal characteristics of a pipe organ may be changed. Even so, the possible number of these variations, though large, is necessarily limited by the available number of stops, or genes; one should not assume that since a certain change has been achieved, infinite change is, therefore, possible.

Since geneticists have gradually been obliged to admit the impossibility of continuous evolution by selecting these common or non-mutational variations which result from recombination of existing factors or genes, they have, therefore, scrutinized very carefully the alternative possibility that such development may result from natural selection

of another kind of rarer and typically more extreme variations, once termed 'sports' by Darwin. Mendel interpreted such sports as lawless floutings of the established germinal order which he discovered. These mysterious germinal changes are now known as mutations; they are believed to arise from sudden inexplicable variations in the number or nature of the individual genes within the chromosomes, rather than in the relationships between genes already existing. Their effect may range from very small-scale, or localized variations (such as in the color of the eye of a fruit fly), on one extreme, to much larger-scale and more highly organized changes on the other, perhaps even to the sudden emergence of a new species. As East, Goldschmidt, and others have observed, however, such changes are always more or less systematic and always involve some degree of coördination of details.

At least five major critiques of evolutionary theory have recently appeared. They are by C. D. Darlington (1), Th. Dobzhansky (5), E. M. East (6), Richard Goldschmidt (9), and C. H. Waddington (24, 25). As previously indicated, Darlington's approach is in terms of emergence of various chemical systems. Dobzhansky emphasizes evidence which indicates that it may sometime be possible to account for the origin of new species in terms of small mutations, but in a group or population rather than on an individual level. East observes that most mutations which have thus far been observed are deleterious and could not serve as an agency for evolution. Also the same mutations may occur again and again, sometimes in reverse direction; they often duplicate environmental changes, all of which indicates that they are really not random, nor are they basic as often supposed, but are incidental to more fundamental systemic

changes. East shows that there is no one-to-one relationship between genes and characters except when all other factors, germinal and environmental, remain constant; and that gene changes are therefore systematic in effect because they must fit into a previously organized system. Also some of these organized changes (as yet unobserved) may possibly be very critical changes which so transform organic forms that evolution is thereby quickly achieved. It seems evident to the writer, however, that although such somatic organization is supposedly new, actually it can only be borrowed from preëxisting organization; it is hard to understand how it could evolve or progress beyond it.

Goldschmidt (9) claims that the extent of possible variation from the observed small-scale mutations, which he calls *micromutations*, is limited in much the same way as are the common variations resulting from recombinations of existing genes, in that the available mutational variations in fundamental form through genic change are finally used up. "Microevolution does not lead beyond the confines of the species" (9, p. 396). On the other hand, he presents rather convincing evidence that another type of more comprehensive or 'large-scale' mutations, which he terms *macromutations*, sometimes occurs. The fact of such macromutations is demonstrated, presumably, by the relatively rapid and broadly-organized evolutionary developments which paleontologists have long observed in the rocks, such as from reptile to bird. While these macromutations are subject to the test of survival or natural selection, just as are micromutations, nevertheless they often show almost at once a fairly complete and balanced working organization of new parts, which appears too quickly to be accounted for by selection alone. Furthermore, they

rapidly bridge the evolutionary gap between radically different characters which could not conceivably be accomplished by gradual transition.

Although most taxonomists seem to recognize the fact of such sudden dramatic emergences, they are perhaps too suggestive of supernatural creation to fit the tough-minded tradition of Darwin and Haeckel, and have, therefore, been largely discounted by many orthodox geneticists.

Goldschmidt has properly been criticized to the effect that regardless of his evidence that macromutations do sometimes occur, he has no good explanation of how and why they occur. While it may be admitted that a sudden change in a gene or hormone, or a new environment, may usher in a previously non-existent life organization, none of these can be the single source of the organization, nor adequate of itself to explain it. Goldschmidt apparently does not seriously attempt to explain in detail what causes such new organizations or why they are balanced and adaptive. He does call attention, however, to the fact that macromutations are only relatively more complex and preadapted than are the recognized micromutations, whose source and nature are also admittedly unexplained; and, therefore, the former are no more mystical or supernaturalistic than the latter. In other words, his argument would seem to be that if organization is to be admitted at all in mutations, why should there not be much as little? The origin of organization is equally mysterious and difficult to explain in either instance.

C. H. Waddington (24, p. 145) of Cambridge University says, anent the problem of the origin of organization, "We must, then accept the existence of different levels of organization as a fact of nature. . . . When elements of a certain degree of complexity become or-

ganized into an entity belonging to a higher level of organization, we must suppose that the coherence of the higher level depends on properties which the isolated elements indeed possessed but which could not be exhibited until the elements entered into certain relations with one another." In other words, the higher level of organization is not vitalistic or supernatural, but is implicit in the units of the lower level, even though there are "properties which the units only exhibit when in combination with each other." Although he does not directly voice his agreement, this seems to be the attitude of Goldschmidt also.

#### IX. INADEQUACIES IN GERMINAL THEORIES

Permeating many discussions of evolution by selection is either uncertainty or else naive preconception about what is selected, which are the specific entities that survive, and what is the selecting agency. It is quite commonly assumed in most discussions that the genes are the surviving entities, and that all other factors are incidental or accessory to their survival. As Goldschmidt insists, however, genes are merely potentialities, even though recent research does indicate that they may be specifically located within the chromosomes; they have no real priority over other potentialities either in the organism or its environment. While a given growth cannot occur in the absence of a given gene, such growth is equally dependent on other potentialities or genes, and many environmental factors as well. No one factor is responsible for the initiation and direction of growth, since these are necessarily functions of the whole rather than of any part. Also, it is just as reasonable to say that there are growth potentialities in the soil or environment, which utilize the elements of the seed or germ cell as a means of self-realiza-

tion or survival, as the conventional reversal of this supposed causal order. New growth potentialities constantly occur in the changing environment; they survive in and through a given germ plasm just as truly as germinal factors survive in the environment. From this viewpoint, obviously, Lamarckianism acquires all the supposed advantages of evidence and logic that Darwinianism now seems to have.

It has long been recognized that there are many vital characteristics which cannot be accounted for by means of a selection theory of any kind. For instance, the behavior of worker and soldier bees and ants, in service of the colony, could not have been developed by natural selection and survival of their sacrificial characteristics in their offspring, because they have no offspring. Any kind of unselfish service to other non-linear individuals is biologically incomprehensible from the selection point of view, especially if it hinders more than it helps the chances of survival of one's own offspring. From the point of view of the insect colony, of course, such sacrificial behavior is so obviously desirable that we tend to accept it as a matter of course, just as we do on the part of the cells and organs of the body. In such instances it is quite evident that we tacitly postulate that such behavior is organized on a higher level of life than that of the specific units considered. The organization on the higher life level could not survive, evidently, except for the fact that the lower level units behave as they do as regards both self and social service. We are apparently obliged to conclude that the system that survives longer is in each case the one on the higher level, which means necessarily that survival and selection are, therefore, always relative to a frame of reference which is always a higher level of organization. Thus the whole influences the part.

Systematic selective adaptation of one form of life to another is conceivable, obviously, when one of the forms is firmly established so as to provide a guide for the other more fluid, transitive form which may adjust to it, like butter to a mould, or skin color to sunshine. But different forms of life must often adapt simultaneously to each other; and often none is provided a dependable guide in any others, since all forms are in a process of genetic flux. This elementary confusion prevails throughout physiology and psychology; as in explaining glandular balance, the mutual adaptation of various insect and flower forms, and the structure and functions of the male and female sex organs. Except for an inclusive level of integration, such mutual adjustments are very difficult to understand.

Human beings are now manipulating the genes of animals and plants in order to develop new varieties most useful to them. From the point of view of the latter, such changes might perhaps be regarded as a Lamarckian effect; since a lineage is being systematically shaped by outside forces. If one reflects, however, it must be admitted that it is equally true that in this situation these creatures are also selectively breeding humans. Since they also benefit and thus favor the survival of the man who is breeding them, these creatures thereby tend to select and develop the kinds of humans that are inclined towards such reciprocal manipulation. Ultimately it is the man-cow symbiosis (the larger organization) which is really selected and evolved, rather than the discrete human or cattle units that comprise it (or man-dog, or crab-anemone, or animal-vegetable symbiosis). Moreover, such interactive selection is obviously achieved just as much through environmental as through germinal means, especially since each creature is environment for the other. Invariably,

it is the larger functional whole which is chiefly selected; selection of parts is secondary and incidental to selection of wholes. As stated by Gerard and Emerson (8, p. 584), "The natural selection of whole integrated systems has led to an evolutionary increase in specialization and integration (coöperation) of the units composing individual's and supra-individuals, both at the biological and social levels."

#### X. GENETIC RELATIONSHIP BETWEEN LIFE LEVELS

The evidence from many sources seems increasingly to demand that we take account in each genetic instance of the possibility that local developments are related to an organization of life on a level higher than that of the individual organism, even though such a hypothesis goes strongly against the grain of our rugged philosophic individualism. Some very reputable and conscientious scientists are inclined to denounce all such considerations as resort to reprehensible mysticism. It seems that for many of us all larger scale descriptions of any sort of phenomena, either organic or inorganic, are relatively suspect of mystical apostasy from pure science—even a knee-jerk reflex if considered as a whole rather than in terms of its constituent tendons and synapses!

In spite of the prevalent prejudice against supra-individualistic interpretations, however, they have evidently not been discontinued or even diminished in recent years. For instance, a symposium, entitled *Levels of Integration in Biological and Social Systems*, was recently presented as a part of the celebration of the fiftieth anniversary of the founding of the University of Chicago. In one of the papers of this symposium, entitled *Higher Levels of Integration*, R. W. Gerard (7, pp. 67-87) reviews recent objective, biological evidence



indicating existence of a social level of integration for various life forms, including amoebae, insects, the colonial coelenterata, and finally man. He concludes, "Man then is an org, an individual organism, and he is also a part of an org, a unit in an epiorganism." It seems possible, at least, that the epiorganism, as conceived by Gerard, the physiologist, may be the agency which organizes Goldschmidt's 'macromutations.'

Actually such a conception is of necessity neither mystical or vitalistic. As Novikoff (20, p. 245) has recently maintained, "The concept of integrative levels . . . neither reduces phenomena of a higher level to those of a lower level as in mechanism, nor describes the higher level in vague non-material terms which are but substitutes for understanding, as vitalism."

It will perhaps be evident that, once a level of life organization above that of the individual is postulated, many of the otherwise conflicting facts of life on the individual level will tend to become unified, and some at least of its many mysteries and paradoxes may be largely resolved. Among these is the strange fact that although most of the motives and activities of the individual are immediately self-serving, and contribute most directly toward self-preservation, at least until the advent of puberty, yet in the long run they are never successful in achieving this end. Death is the final destiny of every organism. But though the individual always dies, the net result of all individual activities of organisms is that their lineage (the inclusive organization) survives and continues to evolve.

As the writer has argued elsewhere (12, p. 98), the life of the individual is both biologically and psychologically meaningless except in its social (or larger biological) setting. While most of the part processes of the organism,

the senses, perception, thinking, and motivation, obviously do serve individual needs and ends for a while, yet this service is really only temporary and is apparently defeated in the long run by the death of the individual. In a larger sense, however, these same individual functions and motives have a larger and longer meaning and service for the life organization of which the individual is a part. Because of the life and death of individuals, the lineage still carries on. This is fact to be reckoned with and not a mere interpretation. Recognition of the actuality of the final social resultant of all life processes is, after all, not so dependent on abstruse reasoning or complex philosophic theory as on good mental hygiene, upon courage to face an obvious biologic and psychologic fact. Such recognition must in the long run profoundly alter, not only our conceptions, but also our attitudes toward all individual functions.

It becomes apparent, therefore, that many of the problems of cellular and individual development, as well as of social or phylogenic development, may be largely solved, or at least reoriented and unified, if higher and lower levels of organization are postulated. Such solutions are limited, of course, rather than final, but so are all scientific solutions. According to Gerard and Emerson (8, p. 585), "Whether the particular mechanisms of evolutions are different at cellular, organismic or societal levels, comparable qualities repeatedly emerge." We recognize readily enough that growth and repair processes within the individual organism are guided by, or at least related to, the needs or processes of the organism as a whole. If that is true, is it not possible that the growth or evolutionary processes of the lineage—that which Gerard calls the epiorganism—are similarly involved and correlated in the adjustments of life on

a higher level of organization? We do not think in terms of mutations of genes when a beard or the sex functions develop in a boy. Very sensibly, although perhaps also very naively and unanalytically, we think of these growths in terms of the part they play in the life history of the organism. In spite of the scientific necessity for attention to details, to the facts of ontogenous growth, why should we not attempt to correlate and interpret them by application of a similar hypothesis in explanation of phylogenetic growth?

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## TOWARDS AN EXPERIMENTAL MEASURE OF PERSONALITY

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### I

As psychology becomes more self-conscious it is with greater frequency that individual workers in the field draw back a sufficient distance from their work to survey their science as a whole. To one who does so, certain shortcomings become apparent that are apt to be overlooked within a special problem. Numerous men have drawn back sufficiently from various phases of psychology to survey a general though yet restricted aspect of the science. Among these, Cattell (2) and Allport (1) have been engaged in such activity. The general needs in the specific study of personality that these and others have found are frequently characteristic of other studies as well. Most reflecting psychologists recognize with Cattell 'the confusion and loss brought about by uncoordinated terminology in the field.' Allport, in his book, offers fifty definitions of Personality which he admits do not cover all the answers offered in the past. That most of these are vague, lacking in precision owing to abstract terminology, is obvious to one who looks at such offerings as "Personality is the sum-total of all biological innate dispositions, impulses, tendencies, appetities, and instincts of the individual, and the acquired dispositions and tendencies—acquired by experience" (1, p. 43), or Allport's own definition, "Personality is the dynamic organization within the individual of those psychophysical systems that determine his unique adjustments to his environment" (1, p. 48).

It is difficult to understand how such definitions can have much experimental

meaning even though those who offer them intend to have personality a measurable category. How one measures 'dynamic organization' or how one is to take a total of 'biological dispositions, impulses, etc.' remains vague. Yet most agree that if personality is to have meaning, it must be measurable.

These, then, are the two shortcomings conspicuous to the psychologist interested in reducing the problem of personality to experimental method: first, the lack of precise defining and its correlative, the lack of standard units used for measurement, such as exist in physics; and secondly, the lack of an experimental framework into which definitions may be set to make them susceptible to measurement. There has been no self-conscious analysis of psychological dimensions which demonstrates either their adequacy or inadequacy, *i.e.*, whether each dimension is necessary, and whether all, taken collectively, are sufficient. A well-developed science requires that its dimensions (1) be necessary and sufficient in the sense that all measurements be expressible in these dimensions and these alone, and (2) be generally accepted so that work by different people at different places can be related. Whereas mass and velocity are terms for which practically all physicists accept a single definition, personality, intelligence, and character are terms for which there are almost as many definitions as there are psychologists.

Cattell has tried to remedy the situation in his own problem by making a definitional matrix of trait unities based on certain historic demands. But, that

his categories are not exhaustive even Cattell realized, for he drew into his classification a miscellaneous category to absorb the leftovers. Jung (8) made similar attempts at classification but admitted that he could not assure the completeness of his results. His efforts were also exclusively based on historic considerations (his case studies and general observations). That a consideration of history recent and past is *necessary* is obvious, for it is history which originally presents the problem. But that history itself has hardly been enough is attested by the definitional matrixes that have been offered.

Whereas Cattell and Jung have attempted a logical classification of fundamental vectors (extracted by both from the history of the problem), Thurstone has attempted to draw these vectors from experiment itself (12). Factors isolated by means of his well-developed statistical techniques are the categories he offers as fundamental. Jung's definitions are hardly experimental but do offer a logical pattern (defective as it is), while Thurstone conversely has all his terms rooted in the experiment but the formal conditions of sufficiency and necessity of dimensions are not made explicit within his method. In order to guarantee that we have derived *all* the fundamental dimensions from our experimental results, we have first to make sure that our method of observation is general enough, and this can only be done by first specifying the dimensions of the science.

The designer of psychological experiments has at least these two demands to satisfy: (1) logical organization, (2) an experimental framework.

The discipline of Logic when applied to history can alone assure us that the list of derived vectors will be exhaustive and cover the entire domain we wish to measure. Logic itself cannot answer any psychological problems, but it does

offer a technique which can make our arrangement of historic demands complete. With this in mind, the writers have made preliminary studies in formal psychology in an effort to build a reference framework such that all experiments can be related, that precise and meaningful categories might be explored, and that *all* that is meaningful will be open for the psychologists' techniques. This design of psychological experiment is presented in its most general form, but it is offered, because even in its present state, it can be of use to the experimenter, and because the suggestions and criticisms of those who approach these results with a fresh view will be of considerable value in its further development.

There has been a marked tendency in the defining of *personality* to become so general that personality comes closer and closer to *psychology* in its meaning. What there is in psychology other than the study of personality is more often than not left vague. Cattell draws emotion and intelligence into the fold of personality, others make character synonymous with it. The difference must be made precise, but before we can talk about personality as a specific category, a general framework of psychological categories will need to be constructed.

In our efforts to build a formal framework for psychology we may start with the concept of 'mind as behavior' (9). Behavior<sup>1</sup> is, in itself, a vague term requiring refinement, for it obviously is a restricted kind of activity of living beings to which we turn when considering mind.

The problem that we shall treat in this paper is primarily concerned with a consideration of that aspect of mind called 'personality.' The point of view that we have adopted here is that an

<sup>1</sup> 'Behavior' is not used here in the limited mechanistic sense by J. B. Watson.



experimental science of personality must begin with an account of the meaning of the problem that will be as complete as possible. Instead of attempting to derive the basic dimensions of personality from empirical results, we first formalize what these basic dimensions should be in order to answer the problem adequately, and then determine how the experiments should be designed. This procedure appears necessary, for the repeated lesson of history has been that a purely empirical methodology is impossible, and when the empirical method is attempted, implicit assumptions are made which make the end results difficult to interpret.

## II

How then shall we define the basic problem of personality? An adequate answer to this question would have to depend upon considering the history of the problem and the manner in which it has influenced our present-day society. Such an undertaking would take us far beyond the scope of this paper and into a subject matter quite different from our original intentions. Instead, we shall suggest how our problem can be defined in such a way as to be in agreement with the intentions of most people working in the field. Our suggestion is that personality be defined as the problem of determining the characteristic ways an individual has of solving his problems. This, be it understood, is simply a preliminary definition which our subsequent defining must attempt to make more precise.

In the first place, what shall we mean by a 'behavior designed to solve a problem'? There are within the literature categories used to describe just such a type of behavior, to wit: 'means' and 'ends.' We say that he who is attempting to solve a problem selects a certain means to accomplish a certain end.

Now, this much translation of the

meaning of problem-solving is very little indeed until we have made explicit some of the basic characteristics of the means-ends relationship.

Ends and means are separable *only* logically, not in fact, for they are correlative, *i.e.*, two aspects of behavior. In any purposeful act a means is employed for an end: the act cannot be understood completely unless both are revealed. Yet like other correlatives, the distinction between them is useful, in fact necessary, for though a specific end always has correlated means, it cannot always have the same means correlated to it, and conversely. With this in mind, it is possible to make more precise what is meant by means and ends. The basic properties of means follow:

(a) Means are actions.

(b) The action is such as to increase the probability of the attainment of the thing which defines the end.

(c) For every end there must always exist at least two distinct means (that is, no means is a necessary condition for attaining the end).

(d) Associated with every means for the attainment of an end there are environmental conditions which themselves are not aspects of the behavior of the individual, but which do assist in the production of the required end; hence no means is *by itself* sufficient for the attainment of any end.

Some explanation of (c) is advisable, for it may not be immediately apparent why a means cannot be a necessary condition for attaining an end.

In the first place, we wish to avoid the absurdity of saying that an individual is solving a problem when his behavior is completely determined by mechanical laws. For example, when an individual is falling, we could hardly say his falling expresses his personality at that moment, or that the behavior he exhibits represents the process of solv-

ing a problem. But mechanically-determined behavior is always such that the individual in a given environment can act in one way, and one way only. To exclude such mechanical interpretations of behavior, it is required that we assert that means are not necessary conditions for the attainment of ends. In effect, we are making a distinction here between *purposive* and *mechanical* behavior, and in a way are ascribing to the individual a certain 'freedom' in his choices. It is not within the scope of this paper to show how such freedom is compatible with a mechanical interpretation of the world. The compatibility of mechanism and purposiveness in nature has adequately been shown in a series of papers by E. A. Singer, Jr. (10, 11).

To understand what is meant by the means-end relationship, let us take a typical situation in which an individual is to be observed: The problem, let us say, is to construct a box adequate for the storage of certain articles. The individual will be observed in an environment in which certain instruments that he can use to solve this problem are available. Our interest, however, is not solely in the instruments he selects, but in the way in which he uses them, that is, the method of application. As a result of such analysis, we will attain a classification of the possible (known) modes of action which can be adopted in a given environment, and which will serve in the solution of his problem. These various modes will constitute the *means*. The *end* will simply be the production of a container of a certain specified type. Making these definitions explicit constitutes devising a morphology of means, and is analogous to a similar problem within biology, the discussion of which, however, we shall momentarily postpone. Generalizing on the idea of an end as brought forth in this illustration, we may define an end as

follows: An end is a property which a number of morphologically dissimilar actions have in common; it is that which holds together or brings sense into a set of actions. The end is really a class of actions or behavior patterns, morphologically dissimilar, which have a common property, that is, a common property of production.

There are two questions we can ask of ends and means respectively which in the most general sense are exhaustive. Other questions asked will be more specialized or asked within the limits of these fundamental ones. We can ask of an individual

1. What means (and ends) *can* he choose (*i.e.*, his potential means), in the sense that the probability of his choosing a given means is greater than zero?

2. What means (and ends) *does* he choose (*i.e.*, his actual means)? A *potential* means for the accomplishment of a certain end (or ends collectively) is any means a given individual could choose for accomplishing the end *independent of environment*, *i.e.*, for all changes in environment. It is important to note that this concept is relative to the individual; it includes only that set of means which the individual could possibly choose in any environment. Thus the automobile was not a potential means for an early Greek even though the materials for making one were available, and is a potential means for a contemporary American, even though he happens to be in a place where no materials are available.

By an *actual* means is meant one that an individual can use in a *given* environment; *i.e.*, actual means are relative to the individual and a particular environment.

These terms are basic to the following definitions:

1. *Knowledge* is the range of *potential* means for a given individual.

2. *Capability* is the range of *actual* means for a given individual.

Hence the measure of knowledge depends upon the history of an individual in the many changing environments in which he solves his problems; the measure of capability is a class product of knowledge and environment; it consists of those *known* means that happen to exist in a specific environment.

3. *Personality* is a measure of the typical inefficiency of a person in the solution of problems, *i.e.*, a measure of the typical choices of inefficient means among the actual means available to him.

We shall want to describe how such measurement might be made experimentally, but before proceeding to do so, it will be advisable to complete the construction of the general framework.

The development on the end side of our scale corresponds to that on the means.

*Potential* ends are those ends which a given individual may pursue (employ means for), independent of environment, *i.e.*, for all changes in environment. Thus, by way of illustration, a visit to Mars is not a potential end for a contemporary individual, whereas it might be for posterity. This concept is also relative to the individual; it includes only that set of ends which the individual could possibly pursue in any environment. He cannot choose (pursue) an end for which there are no means available.

An *actual* end is one that an individual can pursue in a given environment, *i.e.*, actual ends are relative to the individual and a particular environment. The question, "What ends can an individual pursue?" involves the following concept:

*Wisdom* is the range of *potential* ends for a given individual. The question, "What end does he choose?", depends

on the concept of capability, *i.e.*, his range of actual ends.

*Character* is a measure of a person's typical choices of inefficient ends with respect to his ultimate ends.

At this stage, personality and character appear to be identical since any end less than a final end is a means to some more ultimate end. It is necessary, however, in any experiment or set of experiments to have certain temporal and spacial limitations. Relative to these limitations the distinction between means and end is not only possible, but necessary. What are considered as ends in one set of experiments may be considered as means in another, such means in turn implying another end or set of ends.

We now have the following classification-scheme:

	Means	End
Range	Knowledge	Wisdom
Choice	Personality	Character

The four categories offered are the most general ones within a psychology as designed within this paper. These dimensions are taken to be the basic ones. They are basic in the same sense as mass, space, and time are basic within classical mechanics. Here, as within such mechanics, there are also derivative concepts. Though it is not within the scope of this paper to show what such derivatives might be, it would perhaps serve us well to indicate briefly one such development as an example.

One may ask, "The range of an individual's means (or ends) is certainly not a constant. Any individual's means are increased through education of either a deliberate or accidental character. One individual learns more quickly than another; they have different rates of change of means. Is this not an important characteristic?" It is. An individual's *learning* from experience depends on environmental condi-

tions and his innate capacity, *i.e.*, what he brings into the experience in the way of ability to learn.

The 'ability to learn' is what has been traditionally (albeit not consistently) called *intelligence*. This category appears to have been overlooked in the classification we have given, and since intelligence is so often classified under personality traits, it is important that we characterize its rôle from the point of view of the present treatment. One way of regarding personality, as we have defined the term, is the measure of invariant aspects of problem-solving of a given individual in the sense used in factor analysis; that is to say, personality represents that 'constant' which appears in some form in every one of the many different kinds of problems an individual faces. Suppose, now, that instead of considering the typical ways an individual attacks his problems, we consider how rapidly he learns to solve problems, *i.e.*, how rapidly he changes his techniques of problem-solving in repeated tries. We borrow the suggestion of Wyman (13) and define intelligence to be the invariant in all such learning processes of an individual; intelligence represents the 'constant' that appears in all equations descriptive of an individual's *rate* of learning. To borrow a mathematical mode of expression, personality represents the *integral* of the function of problem-solving, intelligence the *first derivative*. Whether or not one experiment could be designed to measure both of these aspects of the psychological individual is a matter for future investigation.

A further note of explanation is needed; the reader might be inclined to ask, "You know how many and what means the individual has, and which he uses, but how do you know how efficiently he uses these means for whatever end he pursues? Is not efficiency in the application of a specific means an

important and fundamental concept?"

The concept of efficiency is already contained in the concept of means. That it is not recognized as such arises from the failure to distinguish between an instrument and a means. An instrument is a physical object which may be used in pursuing an end and may be used with varying degrees of efficiency. But a means is a *type of action* in which the instrument may be a necessary factor. The means is the tool plus its use, and therefore different applications of the tools are different means, though probably enough alike to be included in the same morphological class. There is nothing in the meaning of efficiency of application that is not already included in the concept of means.

### III

We proceed now to the task of constructing the experimental conditions under which personality, as already defined, can be measured. We have said that personality is a measure of typically inefficient choices of ways of solving problems, and we have further argued that every act of problem-solving is made up of three basic aspects: the means, the environment, and the end. This three-fold division suggests the manner in which the original definition of personality can be translated into terms more susceptible to experimental methodology. In the first place, we must define 'type' with respect to choices. That is to say, we must set up criteria by which the experimenter can type certain behavior patterns of the individual. Of the many possibilities, the most fruitful by far seems to be a definition of type based on the kinds of things that *influence* an individual's action. For example, a typical inefficiency in problem-solving arises when an individual is disturbed by a changing environment, even though the environmental changes, for an objec-



tive observer, do not constitute any significant differences in the manner in which the problem should be handled. If we generalize upon this example, we will first inquire: what are the basic dimensions, quantitative changes in which produce a change in an individual's behavior? The three-fold classification we have already given supplies the answer: (1) Does a change in *environment* produce a change in the ways an individual solves his problems? (2) Does a change in the available means produce such a change? (3) Does a change in the end (*i.e.*, in the nature of the problem) produce such a change?

Evidently, the answer to any of these questions would be affirmative in general, for if we make drastic enough changes in any of the three aspects of problem-solving, we will expect an individual to change his ways. However, in general there will be cases where a person is sometimes influenced, and sometimes not. We are actually measuring the *sensitivity* of an individual to such changes, and as in all sensitivity tests, we must not so strengthen the stimulus that all objects are equally affected. Rather, we choose a stimulus-interval within which we obtain some responses, some non-responses.

The translation of the problem of personality is still outside the grasp of the experimenter. In particular, he must know what 'change' means in connection with the three basic dimensions, and knowing the meaning of such change, he must be able to design an experiment to perform the required tests to determine the degree of influence.

In order to 'change' a variable, we require a classification of the possible forms the variable may assume in nature. Our requirements here are more stringent, however, for we regard a change in available means to correspond to a change in a 'stimulus.' Our object is then to discover whether such a

stimulus change does or does not give rise to a response of a certain sort. Sensitivity tests of this kind require not only a classification of stimuli, but also a *scale* or ordering. We must be able to decide whether an 'increase' in the stimulus produces a response or non-response in the subject.

The scale we employ to measure change will depend upon our interests. From the authors' point of view, the most profitable scale one could employ with respect to available means, and to ends, would be a *scale of generality*. One means to an end is more general than another, if, independent of environmental conditions, it solves those problems the other means can solve and others in addition. Thus, the use of the 3-4-5 rule for right triangles is a more *specialized* means than the rule  $a^2 + b^2 = c^2$ . Just how such a scale of generality could be set up for particular experiments is a problem beyond the scope of this paper, but the interested reader can find fruitful suggestions in L. Guttman (6).

If such scales could be devised, then we would say that the available means have been changed if they have become more general (or else more specific). For example, between two problems we might teach the subject certain general formulae of physics, which were unknown to him at the time he attempted to solve the first problem. We would then say that his available means in the second problem were 'changed,' or, in sensitivity terms, that the stimulus had been increased in the direction of generality. Our interest would then be in whether he did or did not respond to the increased stimulus. We note here that we should study the subject's reaction to the increased stimulus both in the case where a positive reaction is efficient, and where it is not. The subsequent discussion will make this point clear.

In a similar fashion, we can change the 'ends,' i.e., the nature of the problem, along a scale of generality, by giving the subject problems of a more general nature, problems whose solutions are more general means for more ultimate ends. Thus, the problem of constructing a machine to form brass cups of sizes within a very wide range would be a more general problem than one of designing a machine to make cups of a specific dimension.

The problem of finding a 'scale' for environmental changes is more complex. There are certainly many ways in which the environment can be changed, ranging from the purely physical changes in temperature, humidity, etc., to the introduction of other individuals, and even social groups. The formulation we have given of the problem of personality does suggest an answer, however. Since personality is a concept dependent upon means and ends, the kind of environmental change that interests us should be a change with respect to these two categories. In particular, we are not concerned with environmental changes that produce physiological responses in an individual that are independent of his purposive activities with respect of his problem-solving. Thus, we are not interested in how he reacts to high temperatures, for these reactions generally are characteristic of his species, and do not throw light upon that aspect of behavior which represents a purpose all his own, namely, the solving of a problem in accordance with his own personality. Rather, our concern should be with those aspects of the environment which represent *other* problems for an individual. If the environment presents for him other problems he could pursue, will he alter his behavior to take these other problems into account? The scale of environmental change will then be a *scale of complexity*, the measure of which will

be the number of other possible problems that an individual could attempt to solve.

It may be that any of the scales we have suggested will have to be varied along several axes, in order to determine completely the measures of personality. If an  $n$ -dimensional system is introduced, however, it will be necessary so to formalize it that it completely characterizes all aspects of change. For example, the scale of complexity of the environment might be divided into a social and a non-social axis, but if this is done, the meaning of the social and non-social will have to be made explicit in such a way that the two categories exhaust the possible ways of changing the environmental complexity.

We now consider the manner in which an experiment would be designed to answer the kind of question we have raised. Suppose, then, that the experimenter has the following information at his disposal:

1. He knows how to change the environmental conditions, the available means, and the end.

2. With respect to any specified problem (end), available means, and environment, he presupposes a knowledge of the most efficient method.

Then the following two sets of experiments can be conducted.

- I. He gives the subject a set of problems the most efficient solution of which is independent of a change in the environment over a particular interval in the environmental scale. He then varies the environment over this interval and observes whether the subject changes his methods. The same type of experiment is conducted for changes in available means, and for changes in ends. In technical terms, the three types of stimuli are varied while the most efficient method of solution is kept invariant.

- II. He gives the subject a set of

problems the most efficient solutions of which are dependent upon changes in environment over a certain interval. He then changes the environment over this interval and notes whether the subject changes his methods. A similar experiment is conducted for changes in available means, and for changes in ends.

A nomenclature for the description of experimental results is convenient, and in the present case is partially supplied by the history of the personality problem; where such a name is lacking, we have resorted to a coinage of terms that seems to be in agreement with the meaning of the result.

Under Experiment I then, we have:

(a) The degree of *extraversion* is the degree of sensitivity an individual exhibits to environmental changes when the most efficient solution remains invariant with such changes.

Having made available to the subject a set of means of various types, and having assigned him a task, he is instructed to use the most effective means in the environment, for accomplishing the job. The environment is 'complicated' by introducing new 'elements,' e.g., other people. By such an introduction new ends (e.g., the subject's social purposes) are incorporated into the environment, *but* the assignment of a specific task has not been altered; his end has not been changed. If, as a result of the complication, he changes his means for a less efficient one, he displays extraversion. The degree of extraversion will depend on how complex the environment has to be made before a less efficient means is chosen (if at all) by the subject. *In this and in all the other measures of personality the experiment must be repeated a number of times with different problems ranging from the point of no response to the point of complete response (if such is attainable).*

(b) The degree of *inconservativism* is the degree of sensitivity to changes in available means when the most efficient solution remains invariant with such changes.

There is made available to the subject a set of means the most efficient of which he is instructed to use for a specified task in a controlled environment. After he has begun 'solving the problem,' new means are introduced that are more general (or more specialized), none of which are more efficient than the one he is using. These changes will continue to be made until the individual changes (if at all) to a less efficient means, thereby measuring the degree of sensitivity, *i.e.*, the degree of *inconservativism*.

(c) The degree of *dispatternizing* is the degree of sensitivity to changes in ends when the most efficient solution remains invariant with such changes.

There is made available to the subject a set of means the most efficient of which he is instructed to use for a specified task in a controlled environment. After he has begun solving the problem he is assigned a new end, more general in nature (*i.e.*, one including the old end) for which the means originally used remains the most efficient in the environment. The generalizing of ends is continued up the scale until the individual changes (if at all) to a less efficient means, thereby measuring the degree of sensitivity, *i.e.*, the degree of *dispatternizing*.

Under Experiment II we have:

(a) The degree of *introversion* is the degree of insensitivity an individual exhibits to environmental changes when the most efficient solution is a function of such changes.

There is made available to the subject a set of means of various types; he is instructed to use the most efficient means for an assigned task. The environment is then 'complicated' by in-

roducing new individuals. The assigned task is not changed but the environmental changes are designed to make one of the other means originally given a more effective one than that originally assigned. The degree of complexity of the environment is increased until the subject chooses the more efficient means, thereby measuring his insensitivity to such change, *i.e.*, the degree of his introversion.

(b) The degree of *conservatism* is the degree of insensitivity an individual exhibits to changes in available means when the efficient solution is a function of such changes.

The individual is assigned a problem for which he chooses a means in a controlled environment. After he has begun solving the problem new means are brought into the environment, means of a more general (or more specialized) nature, which are more efficient than the one he has chosen. These changes are made until the individual chooses (if at all) a more efficient means, thereby measuring his insensitivity to such changes, *i.e.*, his degree of conservatism.

(c) The degree of *patternizing* is the degree of insensitivity an individual exhibits to changes in ends when the most efficient solution is a function of such change.

There is made available to the subject a set of means the most efficient of which he is instructed to use for solving a specified task in a controlled environment. After he has begun solving the problem he is assigned a new task more general in nature for which one or more of the other means available in the environment are more efficient. The generalizing of ends is continued up the scale until the individual changes (if at all) to a more efficient means, thereby measuring his insensitivity to such changes, *i.e.*, his degree of patternizing.

We have discussed the three basic

aspects of each experiment as though they constituted separate investigations. Actually, however, we have a three-variable problem which can be handled by the usual techniques of experimental design. The simplest design would be a factorial  $3 \times 2$  experiment, *i.e.*, an experiment in which each variable is given two values, and all the possible combinations are investigated (4). Such a factorial experiment would enable us to determine the 'significance,' in the sense of the term as used by mathematical statisticians, of the effect, say, owing to environment. We would also be able to determine, by the significance or non-significance of the first-order interaction terms, whether the degree of extraversion is a function of the degree of conservatism, etc. In more complicated designs, the variables would be changed over enough 'points' to enable us to perform a multiple correlation.

By way of clarification, several remarks on the method should be made. In the first place, since we have reduced personality to a problem of sensitivity, it will be necessary, as in all sensitivity tests, to make a number of tests over the scale of the stimulus (3). In any specific case, there may be all sorts of influences on an individual's behavior, and these influences, though uncontrolled by us, are not involved in the measure of his personality. An analogous situation occurs in any sensitivity test: A guinea pig may have died for reasons which are quite independent of the concentration of the drug we have administered. In either case, personality measure or biological assay, it is necessary to make several tests over an interval in the stimulus scale in order to evaluate the results. For further discussion, see Garwood (5), in which it is pointed out that a repetition at a certain point in the stimulus scale is not necessary, so long as we obtain both responses and non-responses.



Next, it may turn out, for example, that an individual shows both a high degree of conservatism, and of incon-servatism. How should such a result be interpreted, and if it occurs, have we not shown that these two terms are not opposites? Consider, for the moment, the behavior of such an individual. In those cases where it is more efficient to select a new means, he tends not to do so; and in those cases where it is not efficient to select a new means, he tends to do so. Would it not be fair to say that such an individual is making no attempt to solve the problem we have presented? In fact, would not a high degree of both conservatism and incon-servatism be an *objective criterion* of whether an individual is seriously attempting to solve the problem? In general, those ends with respect to which an individual shows strong tendencies in both directions (conservatism and incon-servatism, extraversion and introversion, patternizing and dispatternizing) are ends the individual is *not* attempting to attain, whatever may be his verbal assertions.

That there are matters of experimental design which would actually have to be worked out in detail after considerable preliminary research there can be no doubt. The authors' purpose has been primarily a methodological one: to suggest how the problem of personality might be treated experimentally and at the same time treated in such a way as to satisfy the formal demands of a logically closed system.

As Irwin has put it,

"The experimental determination of the voluntary nature of an act becomes in part, then, a matter of the diagnosis of personality. It requires the fullest possible

knowledge of the purposes which an individual exhibits. This is to admit, in view of the scientific study of personality, that it will seldom be easy to make this diagnosis with a high degree of precision. . . . We are ready, however, to rest our case with research men, who do not ask for ease nor for facile answers, but need rather to know how to discover that they are moving in the right direction" (7, p. 135).

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## GEORGE SIDNEY BRETT

1879-1944

With the death of George Sidney Brett in Toronto on October 27, 1944, the world lost one of its great historians of psychology, and Canada its most distinguished philosopher. He was a man of wide interests and many attainments; to his colleagues and students at the University of Toronto, where he taught for 36 years, he seemed equally outstanding as teacher and writer, scholar and administrator. The titles of his publications number over 130, and include books and articles on the history of psychology, the history of science, the history of philosophy, the culture of India, political theory, ethics, literature and religion.

Brett was born in Briton Ferry, South Wales, on August 5, 1879, of English parents, George J. and Emmeline Brett. From 1890 to 1898 he attended Kingswood School at Bath, where he achieved a brilliant record in both classical and scientific studies, winning many prizes and scholarships. For a time he considered medicine as a career; his final decision was probably determined by the award of an Open Exhibition in Classics at Christ Church, Oxford, which he attended from 1898 to 1902.

At Oxford, Brett read for two years in the School of Classical Moderations and two years in the School of Literae Humaniores. In the study of philosophy he finally found a field which challenged his severely disciplined and analytic mind for the rest of his life. As a student, he was influenced chiefly by two superb tutors in Classics and Philosophy at Christ Church, John Alexander Stewart and Herbert W. Blunt; by Thomas Case, President of Corpus Christi College and Waynflete Professor of Moral and Metaphysical Philosophy; by Edward Burnett Tylor, one of

the founders of anthropology; and by John Linton Myres, the eminent classical archeologist. It is not without significance that both Tylor and Myres have constantly emphasized in their numerous publications the importance of the scientific approach in the attempt to understand historical and cultural phenomena.

Early in 1904, Brett entered the Indian Educational Service as Professor of Philosophy in Government College, Lahore, the Punjab. The influence of some four years in India teaching Philosophy and English was deep and lasting. Extending an already wide knowledge of languages, he learned to speak Hindustani and acquired a knowledge of Sanscrit and Arabic. The intellectual traditions of India gave him new material for comparative analysis and induced a lasting appreciation of those thought patterns of the East which philosophers and scientists of the West are often inclined to dismiss too lightly.

He had just completed his book on the *Philosophy of Gassendi* and was already working on his *History of Psychology* when, at the age of twenty-nine, he came from India in September, 1908, to Trinity College in the University of Toronto, as Lecturer in Classics and Librarian. He was appointed Professor of Ethics and Ancient Philosophy the following year, and continued his connection with Trinity until 1921. Shortly after coming to Trinity he also began lecturing in the University. He was an Assistant in the Department of Philosophy from 1909 to 1911; a Lecturer in Greek Philosophy from 1911 to 1916; a part-time professor from 1916 to 1921 and full-time professor in 1921. He was made Acting-Head of the Department of Philosophy in 1926, Head

in 1927, and Professor of Ethics in University College in 1932. He served as Dean of the School of Graduate Studies from 1932 until his death, and as Acting Dean of the Faculty of Arts in 1935-36.

Brett was a member of many learned and scientific societies and of the editorial boards of numerous periodicals including in America the *Journal of General Psychology* and the *International Journal of Ethics*. Among American organizations, perhaps he valued most his connection with the History of Science Society of which he was a charter member. As Chairman of its Publications Committee he read and criticized for the Society many manuscripts dealing with the history of science. The barest chronicle of his activities indicates that his life was a crowded one: It is doubtful if any university has ever had a more prodigious worker, able to carry on his studies, teaching, publishing, and heavy administrative and editorial responsibilities with such apparent ease.

His most outstanding and permanent contributions to scholarship are concerned with the history of psychology. As the value of Brett's work has not always been apparent to many American psychologists who are deeply schooled in the scientific tradition, the present writer has considered it advisable to discuss at some length the presuppositions and methods in terms of which he wrote his three-volume *History of Psychology*. It undertakes to give an account of the general development of psychology in all countries and periods from ancient times through the nineteenth century. Blakey's *History of the Philosophy of Mind*, which appeared in 1848, was apparently the first book that aroused Brett's interest in this field. Later he read Bain's *Body and Mind*, and Sir William Hamilton's edition of Reid's works. Although Ribot

published in 1870 his *Psychologie anglaise contemporaine*, and subsequently a history of German psychology, it was not until some fifteen or twenty years later that the ancient schools first began to receive adequate consideration in Siebeck's *Geschichte der Psychologie*, Chaignet's *Histoire de la psychologie des Grecs*, and Rohde's *Psyche*. Chaignet had undertaken to present a history of ancient psychology, but his work is primarily a history of general philosophy and metaphysics, while Rohde's curious book is concerned mainly with anthropology. Siebeck's book, published in 1887, was the only genuine earlier history of ancient psychology, and Brett acknowledged freely his debt of gratitude to its learning, excellent method, and sound judgment. Only two other general works, Dessoir's *Abriss einer Geschichte der Psychologie* and Klemm's *Geschichte der Psychologie*, had appeared and there was no book at all on the subject in the English language when Brett published, in 1912, the first volume of his *History of Psychology*. What were the influences and circumstances that led him to undertake this massive work?

While a student at Oxford, Brett had attended Thomas Case's lectures on Aristotle's psychology, in which topics such as sensation, memory, habit and intellect were discussed from the point of view of the old 'mental philosophy.' But, owing no doubt to the scientific interest stimulated during the Kingswood days, he seems to have remained restless and dissatisfied with this arid approach. Then he read James' *Principles of Psychology*. It is perhaps not merely a coincidence that his last considerable contribution to scholarship was a chapter entitled, 'The Psychology of William James in Relation to Philosophy,' which appeared in a coöperative volume published in 1942 to commemorate the hundredth anniversary of

James' birth. Under the spell of the greatest of American psychologists Brett began to realize, somewhat dimly at first but very clearly by 1908, that philosophical thought could be studied anew in terms of the expansion of the idea of mind and body. As James emphasized again and again the interaction of philosophy and science through the ages it gradually became more and more obvious to Brett that the locus or middle ground of this intellectual interaction lay between the fields of logic and physiology, that is, in psychology. Henceforth he was interested in the problem of the emergence of psychology from an undifferentiated mass of literature on human nature and social behavior. A re-reading of the great philosophers convinced him that many of their most valuable insights into human nature had been neglected owing to a theological or metaphysical emphasis. As these investigations progressed, he realized also the importance of studying more practical subjects like rhetoric, education, ethics, politics, and theology. The outcome of this vast research was his own monumental history of psychology.

Although the three volumes of this work run to over eleven hundred pages, he stated more than once that, even so, the original manuscript had had to be pruned down severely for purposes of publication. But he made excellent use of the prunings. In fact, one may venture the hypothesis that in certain of his later, more specialized papers on the history of science, and even in the *Government of Man*, he was making excellent use of the vast mass of material which was originally accumulated in the preparation of his greatest book. Such a conclusion would suggest that the work on the history of psychology was the supreme intellectual adventure of his life, but at the same time it would be unsound to assume that Brett was

not interested primarily in philosophy. In a new approach to the history of psychology, it would seem that he had found a field in which could be established, from the first and throughout his life, a fruitful synthesis of interests in philosophy, psychology, medicine, science, and to a lesser extent religion. He was among the first (but he will not be the last) to discover that philosophy, religion, and all the sciences, whether natural or social, have a common meeting ground in psychology.

In the period between 1908 and 1921 when Brett was writing his *History*, the departmental status of psychology was indeterminate; it was no longer a branch of philosophy, but not everyone admitted that it was as yet a branch of science. No one could have appreciated more keenly than Brett the difficulty of defining even the term 'psychology,' a term which refers in ancient and patristic times to the science of the 'soul,' in the late mediaeval and early modern period to the science of 'mind,' in the nineteenth century to the science of 'consciousness,' and more recently to the science of 'behavior.' He maintained that the historian of psychology is not obliged to give a definition: "History alone can adequately unfold the content of the idea denoted by the word 'Psyche' or explain the various meanings that have from age to age been assigned to the science of the soul." Nor would he write the history of psychology from the standpoint of any one of the controversial schools:

"The business of the historian is to record rather than interpret. . . . A history of psychology must not anticipate; it must be a record of beliefs about the soul and of the growth of the human mind in and through the development of those beliefs. Here if anywhere the fundamental axiom is that the evolution of thought is part of the whole evolution. There may ultimately be no evidence that any human



powers have been atrophied or otherwise lost; there may be no ground to believe that any age has had spiritual possibilities greater than our own; but in the process of collecting the data for such judgments it would be a fallacy to presuppose the conclusion."<sup>1</sup>

The historian must therefore record chronologically and impartially the steps by which psychology has reached its present stage of development.

One naturally expects to find in the history of any science an account of the facts which have been discovered and the theories which have been established in that science. But discussions of metaphysical assumptions, methods of enquiry, and principles of interpretation have, until quite recently, occupied a far more prominent place in psychology than the statement of facts, laws or practical applications. In spite of these limitations, psychology in the course of its development has influenced many spheres of human thought; the history of psychology is accordingly very complex and a rigorous selection of material is necessary. In the selection of material and the grouping of data, Brett regarded the nature of man as forming the center of three great lines of interest: "the study of human activities as the psychologist sees them, the study of human life as the doctor looks at it, and the growth of systematic beliefs as reflected in philosophy and religion." An autobiography of the human mind would be given through a union of these

in their historical development. He realized at the outset that such a history would be extremely complicated and almost inextricably entangled with the history of the natural sciences, not to mention all manner of metaphysical and theological speculations. The complications of the history of ideas must be exhibited in the history of psychology, but the historian would not lose his way if the central emphasis were retained:

"The main emphasis is laid on what may be called psychological data in the strict sense; around these data are grouped such theories as diverge from the phenomena of consciousness to derivative doctrines of the soul's antecedents, environment, and future possibilities. The relevant parts of medical and religious theories are regarded as supplementing psychology in two different directions; the treatment of them is sub-ordinated to psychology as the main theme."<sup>2</sup>

With these criteria and considerations before him, Brett undertook the four-fold task of giving for each historical period an account of the state of the sciences which influenced psychology, the state of psychology itself, the influence of psychology upon other sciences, and its general applications. It was clear from the beginning that the history of psychology must be interpreted as a part of the larger history of science. In magnificent prose he has set forth his conception of the permanent educational significance of the whole enterprise:

"A history of a science is a unique species of history. For the content of the science the student may go to the last textbook, where he may learn the established truths without any reference to their genesis or to the men who established them. For those who require no more a history is superfluous: it can add nothing to that knowledge and may be wholly disregarded.

<sup>2</sup> *Ibid.*, p. viii.

<sup>1</sup> *History of psychology*. London: George Allen & Co., 1912, Vol. 1, pp. x, xi. Quotations from the three-volume *History* are given at some length owing to the difficulty of obtaining the set nowadays. In this connection Knight Dunlap has commented: "Brett's book is out of print; not obtainable second-hand; and one or more of the three volumes lost from various libraries. Complete sets of Brett are so invaluable that the library possessing one should keep it under lock and key, and not allow it to be used by any but scholars and exceptional graduate students."

But there is another and a different object for which it has a specific function. If the student is not to be left with the idea that knowledge is a fixed quantity of indisputable facts, if on the contrary he is to acquire a real understanding of the process by which knowledge is continually made and remade, he must learn to look at the movement of ideas without prejudice as a separate fact with its own significance and its own meaning for humanity. To despise forgotten theories because they no longer hold good, and refuse on that account to look backward, is in the end to forget that man's highest ambition is to make progress possible, to make the truth of today into the error of yesterday—in short, to make history."

"Psychology is in some sense a new science, but it has progressed far enough to be conscious of its own claims. It seems, therefore, worth while at this stage to give it the support which may be derived from history. The importance claimed for that history is derived from the ideas expressed above. It is not the kind of importance which belongs either to new discoveries or to antiquarian lore. It is rather the importance that belongs to the great panorama of human effort which it consistently unfolds. However many new psychologies rise and fall, however much the final solution of all problems seems to us to be given only to our own generation, it will still be worth while to contemplate this spectacle of a quest which has called forth from the beginning of time the most passionate desires, the most distorted theories, the most bitter disputes, and the most refined thought possible to the human being. It is not for the historian to utter prophesy, but the eye which surveys the whole course of this subject from its meagre beginnings to its present vastness cannot but anticipate a future growth no less significant and perhaps of incredible importance to the human race."

The most common criticism of Brett, especially among American psychologists, is that he devotes too much space in his *History* to a discussion of purely

epistemological and metaphysical problems. This criticism is unjustified when it is applied to his treatment of psychologists who lived before the age of Wundt. At the same time one must in all fairness admit that Brett's discussion of modern psychologists does tend to be unduly philosophical. His interest in medicine enabled him to appreciate the physiological approach to mind, and his Oxford training made him alive to the cultural and social backgrounds of psychology. But his mind was fundamentally unquantitative: he seems to have had only a flickering interest in the laboratory and statistical techniques that have been developed during the last hundred years. In his treatment of nineteenth century psychologists he is excessively partial to Lotze, Ward and Stout; it would appear that his own philosophical problems and ideals were satisfied by the work of such men. It is true that the founders of laboratory psychology, men like Fechner, Wundt, Ebbinghaus, and various members of the 'Würzburg' school, are considered, but here again Brett is inclined to deal almost entirely with their attitudes towards essentially philosophical questions rather than to inform us regarding their experimental methods and their detailed results.

Why did he not discuss more adequately the experimental psychologists of the modern period? Several answers may be given to this question. Perhaps Brett neglected this phase because he considered that the laboratory tradition had been dealt with so thoroughly in Klemm's masterly work that very little supplementation was required. Then, too, laboratory or 'brass instrument' psychology seems to have bored him even more than it bored James. There was a close resemblance between these two men in another respect: both were so far indifferent to formal distinctions as not to care greatly whether they were

\* *Ibid.*, II, pp. 6-7.

treating psychology philosophically or philosophy psychologically.

A second general criticism of Brett's method refers to his constant tracing of views and theories back to Plato, Aristotle, or Plotinus, accompanied by an almost equal emphasis on the anticipations of modern views in writers like Witelo, Roger Bacon, or Duns Scotus. Is a modern theory explained when it is traced back to Aristotle? Are there any real anticipations in philosophy or psychology? That Brett was utterly unrepentant in this respect becomes clear when one turns to his *Psychology, Ancient and Modern*, published seven years after the *History*. In this later work, under topical headings such as the Physiological Basis, the Analysis of Cognition, the Psychology of Conduct, and Applied Psychology, he analyzes sympathetically and with his customary great learning and subtlety the interrelations between the ancient and modern periods. But more than half of the book is concerned with a discussion of the contribution of Plato and Aristotle to general and applied psychology. In defence of this procedure he maintained that all knowledge of fact, all scientific knowledge is cumulative. The modern achievements (which he would be the last to belittle) could be understood fully only in terms of the philosophical and cultural background from which contemporary scientific psychology has emerged.

Over a third of a century has elapsed since Brett published the first volume of his *History*. Meanwhile over a dozen books dealing with either the general history of psychology or the history of some specialized period or problem have been published by eminent British and American psychologists. In the perspective provided by these more recent studies what is the contemporary estimate of Brett's achievement? Perhaps few living psychologists are better quali-

fied to give an appraisal than Gardner Murphy, who has himself explored the historical background of contemporary psychology, and who has kindly written to the present author as follows:

"The place of Brett's three-volume *History of Psychology* is absolutely secure, no matter how many later histories may be written, for at a time when the English language possessed no book giving historical perspective for psychology he made available the distillation of a lifetime's work, in which not only Plato and Aristotle, but also ancient India; not only the Dark Ages, but the preservation of Greek intellectual treasures throughout their duration; not only the Renaissance of the arts, but also the Renaissance of the scientific spirit, were lovingly portrayed, and presented as background for the understanding of that nineteenth-century experimental psychology in which the methods of natural science were at last systematically applied. In an era in which experimental psychologists tended to repudiate the past, it was Brett first and Brett chiefly, who made available to English-speaking psychologists the historical context and meaning of their subject."

Owing to his prominence during the past twenty-five years as an historian of psychology Brett was honored with invitations to contribute the article in this field to the fourteenth edition of the *Encyclopaedia Britannica* and to participate in international symposia entitled *Feelings and Emotions* and *Psychologies of 1930*. His chapter on "Associationism and 'Act' Psychology" in the latter volume illustrates his historical method at its best. It is doubtful if psychology will ever again have an historian of such great learning and philosophical subtlety.

In addition to his contributions to the history of psychology, Brett published a number of important papers on the history of science. Two of these papers, 'Astronomical Symbolism' and 'The Effect of the Discovery of the

Barometer on Contemporary Thought,' are rich illustrations of the power of the philosophical intelligence to illuminate that type of cultural environment which sustains and fosters the growth of the scientific outlook.

Whether one considers his teaching or his writings, Brett's achievement was characterized by a profound and comprehensive scholarship, an unceasing use of the historical method, and an abiding confidence in the function of reason. In the service of the ideal of comprehensiveness he wrought in many fields: he read and mastered texts in many languages in many periods of his-

tory; he made vast contributions not only to the history of philosophy and psychology, but also to the history of science, literature, politics and religion. His unceasing use of the historical method must have been inspired by a belief in the essential unity of civilization; he did much to establish that unity by exhibiting the logical and historical relationships between philosophical ideas and other great systems of ideas. In his dynamic mind the whole intellectual past of mankind seemed to live again.

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